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PSYCHOLOGY AND PHILOSOPHY

I. DEONTIC LOGIC

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1. So called modal concepts might conveniently be divided into three or four main groups. There are the alethic modes or modes of truth. These are concepts such as the necessary (the necessarily true), the possible (the possibly true), and the contingent (the contingently true). There are the epistemic modes or modes of knowing. These are concepts such as the verified (that which is known to be true), the undecided, and the falsified (that which is known to be false). There are the deontic [1] modes or modes of obligation. These are concepts such as the obligatory (that which we ought to do), the permitted (that which we are allowed to do), and the forbidden (that which we must not do). As a fourth main group of modal categories one might add the existential modes or modes of existence. These are concepts such as universality, existence, and emptiness (of properties or classes).

There are essential similarities but also characteristic differences between the various groups of modalities. They all deserve, therefore, a special treatment. The treatment of the existential modes is usually known as quantification theory. The treatment of the alethic modes covers most of what is traditionally known as modal logic. The epistemic modes have not to any great extent and the deontic modes hardly at all been treated by logicians.

In the present paper an elementary formal logic of the deontic modalities will be outlined.

[1] For the term "deontic" I am indebted to Professor C. D. Broad.

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2. First a preliminary question must be settled. What are the "things" which are pronounced obligatory, permitted, forbidden, etc.?

We shall call these "things" acts.

The word "act", however, is used ambiguously in ordinary language. It is sometimes used for what might be called act-qualifying properties, *e.g.* theft. But it is also used for the individual cases which fall under these properties, *e.g.* the individual thefts.

The use of the word for individual cases is perhaps more appropriate than its use for properties. For the sake of verbal convenience, however, we shall in this paper use "act" for properties and not for individuals. We shall say that theft, murder, smoking, etc. are acts. The individual cases that fall under theft, murder, smoking, etc. we shall call act-individuals. It is of acts and not of act-individuals that deontic words are predicated.

The performance or non-performance of a certain act (by an agent) we shall call performance-values (for that agent). An act will be called a performance-function of certain other acts, if its performance-value for any given agent uniquely depends upon the performance-values of those other acts for the same agent.

The concept of a performance-function is strictly analogous to the concept of a truth-function in propositional logic.

Particular performance-functions can be defined in strict correspondence to the particular truth-functions.

Thus by the negation (-act) of a given act we understand that act which is performed by an agent, if and only if he does not perform the given act. For example: the negation of the act of repaying a loan is the act of not repaying it. If A denotes (is the name of) an act, $\sim A$ will be used as a name of its negation (-act).

Similarly, we can define the conjunction-, disjunction-, implication-, and equivalence-act of two given acts. (The implication-act, *e.g.*, of two given acts is the act which is performed by an agent, if and only if it is not the case that the first act is performed and the second act is not performed by the agent in question.) If A and B denote acts, $A \,\&\, B$ will be used as a name of their conjunction, $A \vee B$ as a name of their disjunction, $A \rightarrow B$ as a name of their implication, and $A \leftrightarrow B$ as a name of their equivalence.

Finally, we can define the tautology- and contradiction (-act) of n given acts. The first is the act which is performed and the second the act which is not performed by an agent, whatever be

the performance-values of the n given acts for the agent in question.

We shall call $\sim A$ the negation-name of A, and $A \,\&\, B$ the conjunction-, $A \vee B$ the disjunction-, $A \rightarrow B$ the implication-, and $A \leftrightarrow B$ the equivalence-name of A and B.

A name of an act which is neither the negation-name of another name of an act, nor the conjunction-, disjunction-, implication-, or equivalence-name of two other names of acts we shall call an atomic name.

By a molecular complex of n names of acts we understand:

(i) Any one of the n names themselves and any one of their negation-names.

(ii) The conjunction-, disjunction-, implication-, and equivalence-name of any two of the n names.

(iii) The negation-name of any molecular complex of the n names, and the conjunction-, disjunction-, implication-, and equivalence-name of any two molecular complexes of the n names.

The n names are called constituents of their molecular complexes. If they are atomic names, they are called atomic constituents.

As to the use of brackets we adopt the convention that the symbol & has a stronger combining force than $\vee$, $\rightarrow$, and $\leftrightarrow$; the symbol $\vee$ than $\rightarrow$ and $\leftrightarrow$; and the symbol $\rightarrow$ than $\leftrightarrow$. Thus, *e.g.*, we write for $(((A \,\&\, B) \vee C) \rightarrow D) \leftrightarrow E$ simply $A \,\&\, B \vee C \rightarrow D \leftrightarrow E$.

The symbols $\sim$, &, $\vee$, $\rightarrow$, and $\leftrightarrow$ will be used for truth-functions as well as for performance-functions. This ambiguity does not easily lead to confusion and is, therefore, to be preferred to the introduction of two special sets of symbols.

3. As an undefined deontic category we introduce the concept of permission. It is the only undefined deontic category which we need.

If an act is not permitted, it is called forbidden. For instance: Theft is not permitted, hence it is forbidden. We are *not allowed to* steal, hence we *must not* steal.[1]

If the negation of an act is forbidden, the act itself is called obligatory. For instance: it is forbidden to disobey the law, hence it is obligatory to obey the law. We *ought to* do that which we are *not allowed not to* do.

If an act and its negation are both permitted, the act is called

[1] It need hardly be stressed that the question of validity of various deontic propositions (other than those which are true on formal grounds) does not concern us in this paper.

(morally) indifferent. For instance: in a smoking compartment we may smoke, but we may also not smoke. Hence smoking is here a morally indifferent form of behaviour.

It should be observed that indifference is thus a narrower category than permission. Everything indifferent is permitted, but everything permitted is not indifferent. For, what is obligatory is also permitted, but not indifferent.

(The difference between the permitted and the indifferent among the deontic modes is analogous to the difference between the possible and the contingent among the alethic modes.)

The above deontic concepts apply to a single act (or performance-function of acts). There are also deontic concepts which apply to pairs of acts.

Two acts are morally incompatible, if their conjunction is forbidden (and compatible if it is permitted). For instance: giving a promise and not keeping it are (*morally*) incompatible acts.

Doing one act commits us to do another act, if the implication of the two acts is obligatory. For instance: giving a promise commits us to keep it.

The proposition that the act named by A is permitted will be expressed in symbols by PA.

The proposition that the act named by A is forbidden, is the negation of the proposition that it is permitted. It can thus be symbolized by $\sim(P\,A)$.

The proposition that the act named by A is obligatory, is the negation of the proposition that the negation of the act is permitted. It can thus be symbolized by $\sim(P \sim A)$. We shall also use the shorter expression $O\,A$.

The proposition that the act named by A is (morally) indifferent can be symbolized by $(P\,A) \,\&\, (P \sim A)$.

The proposition that the acts named by A and by B are (morally) incompatible can be symbolized by $\sim(P\,A \,\&\, B)$.

The proposition that the performance of the act named by A commits us to perform the act named by B can be symbolized by $O\,A \rightarrow B$. But $O\,A \rightarrow B$ means the same as $\sim(P \sim (A \rightarrow B))$, and this means the same as $\sim(P\,A \,\&\, \sim B)$. Commitment can thus be explained in terms of compatibility.

P and O are called the deontic operators. Sentences of the type "P ", where a name of an act (or a molecular complex of names of acts) has to be inserted in the blank, we shall call P-sentences. Similarly, we shall call sentences of the type "O " O-sentences.

As to the use of brackets it should be remarked that P- and

O-sentences as constituents of molecular complexes of sentences should be enclosed within brackets in order to avoid confusion. It should further be observed that a deontic operator before a molecular complex of names of acts refers to the whole complex and not to its first constituent only. Thus, *e.g.*, $P\,A \vee B$ means that the act named by $A \vee B$ is permitted.

The system of Deontic Logic, which we are outlining in this paper, studies propositions (and truth-functions of propositions) about the obligatory, permitted, forbidden, and other (derivative) deontic characters of acts (and performance-functions of acts).

We shall call the propositions which are the object of study deontic propositions. The sentences, in which they are expressed in our system, are P- and O-sentences or molecular complexes of such sentences.

4. A task of particular importance which Deontic Logic sets itself is to develop a technique for deciding, whether the propositions it studies are logically true or not. (The decision problem.)

Sometimes molecular complexes of P- and O-sentences express truths of logic for reasons which have nothing to do with the specific character of deontic concepts. For instance: If A is permitted, if B is permitted, then B is forbidden, if A is forbidden. In symbols: $((P\,B) \to (P\,A)) \to (\sim (P\,A) \to \sim (P\,B))$. This is a truth of logic. It is an application of a variant of the so called *modus tollens* which is valid for any sentences, whether deontic or not. It is, therefore, a trivial truth from the point of view of our Deontic Logic.

Sometimes, however, molecular complexes of P- and O-sentences express truths of logic for reasons which depend upon the specific (logical) character of deontic concepts. For instance: If A is obligatory and if doing A commits us to do B, then B is obligatory too. In symbols: $(O\,A)\,\&\,(O\,A \to B) \to (O\,B)$. It is intuitively obvious that this is a truth of logic, *i.e.* something which is valid on purely formal grounds. It is, however, not an application of any scheme which is valid for *any* sentences, whether deontic or not. The existence of logical truths which are peculiar to deontic concepts is what makes the study of Deontic Logic interesting.

If a molecular complex of P- and O-sentences expresses logical truth for reasons which are independent of the specific nature of deontic concepts, then its truth can be established or proved in a truth-table of propositional logic.

If, however, a molecular complex of P- and O-sentences

expresses logical truth for reasons which depend on the specific nature of deontic concepts, then its truth cannot be established by the means of propositional logic alone. The question therefore arises: What is the necessary and sufficient criterion which a molecular complex of *P*- and/or *O*-sentences must satisfy in order to express a logically true proposition?

5. Let us call "permitted" and "forbidden" the two deontic values.

An act will be called a deontic function of certain other acts, if the deontic value of the former uniquely depends upon the deontic values of the latter.

It is easy to see that not any act which is a performance-function of certain other acts is also a deontic function of them. (Otherwise the logic of deontic concepts would be trivial.)

Consider first the negation of a given act. From the fact that A is performed, we can conclude to the fact that $\sim A$ is not performed. But from the fact that A is permitted, we can conclude nothing as to the permitted or forbidden character of $\sim A$. Sometimes $\sim A$ is permitted, sometimes not. If A is what we have called indifferent, then $\sim A$ is also permitted, but if A happened to be obligatory as well as permitted, then $\sim A$ would be forbidden. In the smoking compartment, *e.g.*, not-smoking is permitted and also smoking. But in the non-smoking compartment, not-smoking is permitted and smoking forbidden.

Consider next the conjunction of two acts. From the fact that A and B are both performed, it follows that $A \,\&\, B$ is performed. But from the fact that A and B are both permitted, it does not follow that $A \,\&\, B$ is permitted. Sometimes $A \,\&\, B$ is permitted, sometimes not. For, A and B may both be permitted, but doing either of them may commit us not to do the other. I may be free to promise and also not to promise to give a certain thing to a person, and free to give and also not to give this thing to him, but forbidden to promise to give and yet not give it.

Consider, finally, the disjunction of two acts. From the fact that at least one of the two acts A and B is performed, it follows that $A \vee B$ is performed, and from the fact that none of the two acts A and B is performed, it follows that $A \vee B$ is not performed. Similarly, from the fact that at least one of the acts is permitted, it follows that their disjunction is permitted, and from the fact that both acts are forbidden, it follows that their disjunction is forbidden. In other words: the disjunction of two acts is permitted, if and only if at least one of the acts is permitted. Speaking loud or smoking is permitted in the reading-room, if

and only if speaking loud is permitted or smoking is permitted.[1]

Thus deontic functions are similar to performance-functions (and truth-functions) in regard to disjunction, but not similar in regard to negation and conjunction. The similarity can be laid down as a Principle of Deontic Distribution:

If an act is the disjunction of two other acts, then the proposition that the disjunction is permitted is the disjunction of the proposition that the first act is permitted and the proposition that the second act is permitted.

(This principle can, naturally, be extended to disjunctions with any number n of members.)

In virtue of familiar principles of formal logic, any molecular complex of n names of acts has what we propose to call a perfect disjunctive normal form. This is a 0-, 1-, or more-than-1-termed disjunction-name of n-termed conjunction-names. Each of the n original names or its negation-name occurs in every one of the conjunction-names.

In virtue of the above Principle of Deontic Distribution, any molecular complex of n names of acts denotes a deontic function of the acts denoted by the conjunction-names in its perfect disjunctive normal form.

Consider now a P-sentence $P\,c$, where c stands for (an atomic name of an act or) a molecular complex of names of acts. Let $c_1, \ldots c_k$ stand for the conjunction-names in the perfect disjunctive normal form of c. The sentences $P\,c_1, \ldots, P\,c_k$ we shall call the P-constituents of $P\,c$.

Since, in virtue of the Principle of Deontic Distribution, c denotes a deontic function of the acts named by $c_1, \ldots, c_k$, it follows that $P\,c$ expresses a truth-function of the propositions expressed by $P\,c_1, \ldots, P\,c_k$. Generally speaking: a P-sentence expresses a truth-function of the propositions expressed by its P-constituents.

Consider n names of acts $A_1, \ldots, A_n$. There are in all 2^n conjunction-names which can be formed by selecting m ($0 \leqq m \leqq n$) out of the n names and taking the negation-names of the

[1] The meaning of "or" in ordinary language is not quite settled. When we say that we are permitted to do *A or B*, we sometimes mean, by implication, that we are allowed to do both. Sometimes, however, we mean that we are allowed to do one and one only of the two acts. Which meaning the "or" conveys by implication depends upon the material nature of the individual case, in which it is used. It ought to be stressed that our use of "or" in this paper is neutral with regard to such material differences in the individual situations. That we are permitted to do *A or B* means here that we are permitted to do at least one of the two acts, and neither excludes nor includes, by implication, the permission to do both.

remaining n-m names. (The order of the names in a conjunction-name is irrelevant.) By the deontic units in the deontic realm of the acts named by $A_1, \ldots, A_n$ we shall understand the propositions that the respective acts named by those 2^n conjunction-names are permitted. By the deontic realm itself we shall understand the disjunction of all the deontic units.

Thus, *e.g.*, the deontic units of the deontic realm of the sole act named by A are the propositions expressed by $P\,A$ and $P \sim A$. The deontic realm itself is the proposition expressed by $(P\,A) \vee (P \sim A)$. The deontic units of the deontic realm of the acts named by A and B are the propositions expressed by $P\,A \,\&\, B$ and $P\,A \,\&\, \sim B$ and $P \sim A \,\&\, B$ and $P \sim A \,\&\, \sim B$. Etc.

The deontic units of the deontic realm of given acts are logically independent of one another, meaning that they can be true or false in any combination of truth-values. There is, however, one point at which this independence might be questioned. Could *all* the deontic units be false ?

Let A be the name of an act. That all (both) the deontic units in the deontic realm of this act are false means that the act itself and its negation are both forbidden. In symbols: $\sim (P\,A) \,\&\, \sim (P \sim A)$. Since the act *or* its negation is performed by any agent whenever he acts, the falsehood of all the deontic units means that we are forbidden to act in any way whatsoever.

Is such a prohibition illogical ? Its counterpart in the logic of the alethic modalities would be the case, when a proposition and its negation are both impossible, and its counterpart in the logic of the epistemic modalities would be the case, when a proposition and its negation are both known to be false. *These* cases are obviously logical impossibilities. On the other hand, in the logic of the existential modalities the corresponding case is, when a property and its negation are both empty. *This* is not an impossibility, since the Universe of Discourse may have no members. The question, therefore is, whether the deontic modes at this point resemble the alethic and the epistemic modes or whether they resemble the existential modes.

Ordinary language and our common sense logical intuitions seem at first not to provide us with a clear answer. A simple logical transformation, however, will help us to make up our minds.

That the negation of an act is forbidden means that the act itself is obligatory. Thus we can for $\sim (P \sim A)$ write $O\,A$. That an act and its negation are both forbidden means the same as that the act itself is both obligatory and forbidden.

At this point an appeal to ordinary language will, I think, be decisive. We seem prepared to reject a use of the words, according to which one and the same act could be truly called both obligatory and forbidden.[1] If, however, we reject this use, we must also reject the idea that all the units in a deontic realm could be false.

Thus, on the point at issue, the deontic modalities appear to resemble the alethic and the epistemic modalities rather than the existential ones.

The restriction on the logical independence of the deontic units, which we are forced to accept, can be laid down as a Principle of Permission :

Any given act is either itself permitted or its negation is permitted.

There are alternative formulations of the principle. We might also have said : If the negation of an act is forbidden, then the act itself is permitted. And this again is equivalent to saying : If an act is obligatory, then it is permitted.

6. Which truth-function of its P-constituents a P-sentence expresses can be investigated and decided in truth-tables.

We shall here construct a truth-table for the following P-sentences : $P\,A$ and $P \sim A$ and $P\,A \,\&\, B$ and $P\,A \vee B$ and $P\,A \rightarrow B$ and $P\,A \leftrightarrow B$ and $P\,A \vee \sim A$. The perfect disjunctive normal form of A (in terms of A and B) is $A \,\&\, B \vee A \,\&\, \sim B$. The normal form of $\sim A$ is $\sim A \,\&\, B \vee \sim A \,\&\, \sim B$.

The table looks as follows :

$P\,A \,\&\, B$	$P\,A \,\&\, \sim B$	$P \sim A \,\&\, B$	$P \sim A \,\&\, \sim B$	$P\,A$	$P \sim A$	$P\,A \,\&\, B$	$P\,A \vee B$	$P\,A \rightarrow B$	$P\,A \leftrightarrow B$	$P\,A \vee \sim$
T	T	T	T	T	T	T	T	T	T	T
T	T	T	F	T	T	T	T	T	T	T
T	T	F	T	T	T	T	T	T	T	T
T	T	F	F	T	F	T	T	T	T	T
T	F	T	T	T	T	T	T	T	T	T
T	F	T	F	T	T	T	T	T	T	T
T	F	F	T	T	T	T	T	T	T	T
T	F	F	F	T	F	T	T	T	T	T
F	T	T	T	T	T	F	T	T	T	T
F	T	T	F	T	T	F	T	T	F	T
F	T	F	T	T	T	F	T	T	T	T
F	T	F	F	T	F	F	T	F	F	T
F	F	T	T	F	T	F	T	T	T	T
F	F	T	F	F	T	F	T	T	F	T
F	F	F	T	F	T	F	F	T	T	T

[1] For the " relativity " of deontic propositions cf. below, p. 15.

The normal form of $A \,\&\, B$ is $A \,\&\, B$. The normal form of $A \vee B$ is $A \,\&\, B \vee A \,\&\, \sim B \vee \sim A \,\&\, B$. The normal form of $A \rightarrow B$ is $A \,\&\, B \vee A \,\&\, B \vee \sim A \,\&\, \sim B$. The normal form of $A \longleftrightarrow B$ $A \,\&\, B \vee \sim A \,\&\, \sim B$. The normal form of $A \vee \sim A$ is $A \,\&\, B \vee A \,\&\, \sim B \vee \sim A \,\&\, B \vee \sim A \,\&\, \sim B$. Thus the seven P-sentences have in all four P-constituents, *viz.* $P\,A \,\&\, B$ and $P\,A \,\&\, \sim B$ and $P \sim A \,\&\, B$ and $P \sim A \,\&\, \sim B$. They express the deontic units of the deontic realm of the two acts named by A and by B.

In distributing truth-values over the deontic units (or the P-constituents) we have to observe the restriction imposed by the Principle of Permission. The subsequent calculation of truth-values for the seven deontic propositions (or the seven P-sentences) depends only on the Principle of Deontic Distribution.

What is the truth-table for $P\,A \,\&\, \sim A$? The perfect disjunctive normal form of $A \,\&\, \sim A$ is "empty", *i.e.* a O-termed disjunction. Thus $P\,A \,\&\, \sim A$ too is a O-termed disjunction of P-constituents. It might be argued that a disjunction is true, if and only if at least one of its members is true, and that a O-termed disjunction, since it has no members, is never true (always false). If, however, $P\,A \,\&\, \sim A$ is always false, its negation $\sim (P\,A \,\&\, \sim A)$ is always true. But $\sim (P\,A \,\&\, \sim A)$ means the same as $O\,A \vee \sim A$. Thus, on the above criterion for the truth of a O-termed disjunction, it follows that $O\,A \vee \sim A$ is a deontic tautology.

It might, however, be questioned whether it can be regarded as a truth of logic that a tautologous act is obligatory (and a contradictory act forbidden). The corresponding proposition in the logic of the alethic modalities is that a tautologous proposition is necessary (and a contradictory proposition impossible), and the corresponding proposition in the logic of the existential modalities is that a tautologous property is universal (and a contradictory property empty). *These* corresponding cases are obviously logical truths. On the other hand, the corresponding propositions in the logic of the epistemic modalities is that a tautologous proposition is verified (and a contradictory proposition falsified). *This* is not a logical truth. For, a proposition may be tautologous (contradictory) without our knowing it. The question therefore is, whether the deontic modes at this point resemble the alethic and the existential modes, or whether they resemble the epistemic modes.

Ordinary language and our common sense logical intuitions seem not to provide us with any clear answer. It appears, moreover, that no further logical considerations can help us to

decide on the issue. It may be thought "awkward" to permit contradictory actions [1] but it is difficult to conceive of any logical argument against this permission. From the point of view of logic, therefore, the most plausible course seems to be to regard $P\,A\,\&\sim A$ and $O\,A \vee \sim A$ as expressing contingent propositions which can be either true or false.

Thus, on the point at issue, the deontic modalities appear to resemble the epistemic rather than the alethic and the existential modalities.

We suggest the following Principle of Deontic Contingency: *A tautologous act is not necessarily obligatory, and a contradictory act is not necessarily forbidden.*

7. Let us consider a molecular complex of *P*- and/or *O*-sentences. *O*-sentences can be regarded as abbreviations for negation-sentences of certain *P*-sentences. (Cf. above, p. 8.) If the molecular complex happened to contain *O*-sentences, we replace them by negation-sentences of *P*-sentences. Thus we get a new molecular complex, all the constituents of which are *P*-sentences.

We now turn our attention to the (molecular complexes of) names of acts which follow after the modal operators in this new molecular complex of *P*-sentences. We make an inclusive list of all atomic names which are constituents of at least one of the (molecular complexes of) names of acts in question. Thereupon we transform these (molecular complexes of) names of acts into their perfect disjunctive normal forms in terms of all atomic names which occur in our list. The respective conjunction-names in these normal forms preceded by the deontic operator *P* we shall call the *P*-constituents of the initially given molecular complex of P- and/or *O*-sentences. (Cf. the example given below).

We know already that any *P*-sentence expresses a truth-function of the propositions expressed by its *P*-constituents. Since any molecular complex of *P*- and/or *O*-sentences expresses a truth-function of the propositions expressed by the *P*- and/or *O*-sentences themselves, it follows that any molecular complex of *P*- and/or *O*-sentences expresses a truth-function of the propositions expressed by its *P*-constituents.

Which truth-function of the propositions expressed by its *P*-constituents a molecular complex of *P*- and/or *O*-sentences expresses can be investigated and decided in a truth-table.

[1] Contradictory acts should not be confused with (morally) indifferent acts. The former are acts which, by definition, are never performed by an agent. The latter are acts which we are permitted to perform, but also not to perform.

This fact constitutes a solution of the decision problem for the system of Deontic Logic which we are outlining in this paper.

The technique of constructing truth-tables in Deontic Logic will be illustrated by an example.

Let the molecular complex be $(O\,A)\,\&\,(O\,A \to B) \to (O\,B)$. (Cf. above, p. 5.)

$O\,A$ is an abbreviation for $\sim(P \sim A)$, $O\,A \to B$ for $\sim(P\,A\,\&\sim B)$, and $O\,B$ for $\sim(P \sim B)$. By replacing O-sentences by P-sentences in our initial complex, we get the new complex $\sim(P \sim A)\,\&\sim(P\,A\,\&\sim B) \to \sim(P \sim B)$.

The atomic names of acts which are constituents of (at least one of) the molecular complexes "inside" the operator P are A and B. The perfect disjunctive normal form of $\sim A$ in terms of A and B is $\sim A\,\&\,B \vee \sim A\,\&\sim B$. The normal form of $A\,\&\sim B$ is $A\,\&\sim B$. The normal form of $\sim B$ is $A\,\&\sim B \vee \sim A\,\&\sim B$. The P-constituents of the initially given molecular complex, therefore,are $P \sim A\,\&\,B$ and $P\,A\,\&\sim B$ and $P \sim A\,\&\sim B$.

Since the P-constituents do not represent all the deontic units of the deontic realm of the acts named by A and by B (cf. above p. 10) the Principle of Permission does not here impose any restrictions upon the combinations of truth-values. The calculation of truth-values depends only upon the Principle of Deontic Distribution (and principles of propositional logic). The table looks as follows:

$P\,A\&\sim B$	$P \sim A\&B$	$P \sim A\&\sim B$	$O\,A$	$O\,A \to B$	$(O\,A)\&(O\,A \to B)$	$O\,B$	$\to$
T	T	T	F	F	F	F	T
T	T	F	F	F	F	F	T
T	F	T	F	F	F	F	T
T	F	F	T	F	F	F	T
F	T	T	F	T	F	F	T
F	T	F	F	T	F	T	T
F	F	T	F	T	F	F	T
F	F	F	T	T	T	T	T

It is seen that the molecular complex which we are investigating (indicated by "$\to$" in the column to the extreme right) expresses the tautology of the propositions expressed by its P-constituents.

8. A molecular complex of P- and/or O-sentences which expresses the tautology of the propositions expressed by its P-constituents, is said to express a truth of Deontic Logic or a deontic tautology.

A (true) proposition to the effect that a certain molecular complex of *P*- and/or *O*-sentences expresses a deontic tautology will be called a law of Deontic Logic.

We mention below some examples of such laws. When we call two molecular complexes of *P*- and/or *O*-sentences identical, we mean that their equivalence-sentence expresses a deontic tautology. When we say that (the proposition expressed by) one molecular complex of *P*- and/or *O*-sentences entails (the proposition expressed by) another, we mean that their implication-sentence expresses a deontic tautology. The propositions expressed by the molecular complexes of sentences given below (or by the equivalence- or implication-sentences in question) are easily shown by truth-tables to be tautologies.

(i) Two laws on the relation of permission to obligation, and *vice versa*:

a $P\,A$ is identical with $\sim(O \sim A)$, *i.e.* $(P\,A) \leftrightarrow \sim(O \sim A)$ expresses a deontic tautology.

b $O\,A$ entails $P\,A$, *i.e.* $(O\,A) \rightarrow (P\,A)$ expresses a deontic tautology.

The second of these laws should not be confused with the above (alternative formulation of the) Principle of Permission (p. 9). In the proof of (i)*b* this principle is already assumed.

(ii) Four laws for the " dissolution " of deontic operators:

a $O\,A \,\&\, B$ is identical with $(O\,A) \,\&\, (O\,B)$.

b $P\,A \vee B$ is identical with $(P\,A) \vee (P\,B)$.

c $(O\,A) \vee (O\,B)$ entails $O\,A \vee B$.

d $P\,A \,\&\, B$ entails $(P\,A) \,\&\, (P\,B)$.

The second of these laws should not be confused with the Principle of Deontic Distribution (p. 7). In the proof of (ii)*b* this principle is already assumed.

(iii) Six laws on " commitment ":

a $(O\,A) \,\&\, (O\,A \rightarrow B)$ entails $O\,B$. If doing what we ought to do commits us to do something else, then this new act is also something which we ought to do. (This was the example of a deontic tautology which we discussed above.)

b $(P\,A) \,\&\, (O\,A \rightarrow B)$ entails $P\,B$. If doing what we are free to do commits us to do something else, then this new act is also something which we are free to do. In other words: doing the permitted can never commit us to do the forbidden.

c $\sim(P\,B) \,\&\, (O\,A \rightarrow B)$ entails $\sim(P\,A)$. This is but a new version of the previous law. If doing something commits us to do the forbidden, then we are forbidden to do the first thing. For instance: if it is obligatory to keep one's promises and if we

promise to do something which is forbidden, then the act of promising this thing is itself forbidden.

d $(O\,A \to B \vee C) \,\&\, \sim (P\,B) \,\&\, \sim (P\,C)$ entails $\sim (P\,A)$. This is a further version of the two previous laws. An act which commits us to a choice between forbidden alternatives is forbidden.

e $\sim ((O\,A \vee B) \,\&\, \sim (P\,A) \,\&\, \sim (P\,B))$. It is logically impossible to be obliged to choose between forbidden alternatives.[1]

f $(O\,A) \,\&\, (O\,A \,\&\, B \to C)$ entails $O\,B \to C$. If doing two things, the first of which we ought to do, commits us to do a third thing, then doing the second thing alone commits us to do the third thing. "Our commitments are not affected by our (other) obligations."

g $O \sim A \to A$ entails $O\,A$. If failure to perform an act commits us to perform it, then this act is obligatory.

The truth of all these laws follows from our intuitive notions of obligation and permission. Not all of the laws themselves, however, are intuitively obvious. In the case of some of the laws, moreover, it is not intuitively clear whether their truth is a matter of logic or a matter of moral code. This proves that the decision procedure of Deontic Logic which we have outlined is not void of philosophical interest.

Any molecular complex of *P*- and/or *O*-sentences has what we propose to call an absolutely perfect disjunctive normal form. This we get by replacing every one of the *P*- and/or *O*-sentences by a disjunction-sentence of *P*-constituents of the complex and transforming the molecular complex of *P*-sentences thus obtained into its perfect disjunctive normal form. If the normal form contains the conjunction of the negation of all *P*-constituents, we omit it from the normal form.

The absolutely perfect disjunctive normal form shows with which ones of the possible combinations of truth-values in its *P*-constituents the molecular complex in question expresses agreement and with which ones it expresses disagreement. If it agrees with all possibilities, it expresses a deontic tautology, *i.e.* is a truth of Deontic Logic.

[1] Aquinas several times refers to the laws *d* and *e*. He distinguishes between a man's being *perplexus simpliciter* and his being *perplexus secundum quid*. The former is the case, if he is, as such, obliged to choose between forbidden alternatives. The latter is the case, if he by a previous wrong act commits himself to a choice between forbidden alternatives. Aquinas rightly denies that a man could be *perplexus simpliciter* (*e*) and affirms that a man might be *perplexus secundum quid* (*d*). Cf. *De Veritate*, Q. 17, art. 4; *Summa Theologica*, jajjar, Q. 19, art. 6; *Summa Theologica*, iii*, Q. 64, art. 6. For these observations I am indebted to Mr. P. Geach.

9. There is one relevant respect, in which the deontic modalities differ from the alethic, epistemic, and existential modalities. It can be illustrated as follows: If a proposition is true, then it is possible, and if a proposition is true, then it is not falsified, and if a property is true of a thing, then the property exists. But if an act is performed (or not performed), then nothing follows as regards its obligatory, permitted or forbidden character. There is thus an important sense in which the deontic modalities unlike the alethic, epistemic, and existential ones have no logical connexions with matters of fact (truth and falsehood). This is a point about deontic categories which has often been stressed by moral philosophers.

10. In this paper deontic propositions have been treated as "absolute". They can, however, be made "relative" in several ways.

First of all, it might be argued that deontic propositions are sometimes, or perhaps always, relative to some so-called moral code. What is obligatory within one moral code, may be forbidden within another.

Secondly, instead of simply considering whether an act is obligatory, permitted or forbidden, we may consider propositions of the following type: x is permitted to do A, or x permits y to do A. Introducing quantifiers we then get propositions of the type: somebody is permitted to do A, or somebody permits everybody to do A, etc. The logical systems which we get by such extensions are of considerable complexity. Their decision-problem can be solved for many interesting cases, but not for all cases.

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II.—INDUCTION AND SCIENTIFIC METHOD

By G. Buchdahl

The problem of inductive inference is one of those time-honoured enquiries to which logicians seem to return periodically. The formulation of this type of inference encountered most commonly and which we may call the "classical formulation", runs as follows. We observe a *finite* set of instances having in common a number of properties *a*, *b*, *c*, . . . and also the property *p*. In addition there may be a number of properties possessed by some of the instances but not others. We *conclude* or wish to conclude that *everything* which possesses the set *a*, *b*, *c* . . . also possesses p. The problematic aspect of this type of inference lies in the step from a finite to an infinite set of instances.

The classical formulation of induction is not the only possible one. Indeed, more particularly it describes so-called direct generalisation which is often held to be a relatively crude form of induction. Nevertheless it has the merit of drawing our attention to a fundamental aspect of all forms of induction which is that of "expansion"—of going to something more, something seemingly new, of getting "new knowledge from old", as Jevons once said.[1]

The correctness or uniqueness of the classical formulation may of course be questioned. In fact, the difficulties surrounding it are well illustrated by the fact that some writers deny even its very existence. Thus W. R. Boyce Gibson [2] maintained that "strictly speaking" there is no such thing as inductive inference but only a complex inductive method consisting of observation and analysis, hypothesis and deduction, verification and systematization. In such a scheme inductive inference as here defined has little place; we only "conjecture" that a certain hypothesis may be true. This view stresses the important fact that frequently there may not be a single point at which we suddenly and miraculously move from observation to conclusion. (It is another question whether or not we ever move at all. We shall return to this subsequently. At present it will be as well to maintain that there must be an inference somewhere in this process—for instance, at the point where we confirm an hypothesis.)

[1] W. S. Jevons, *The Principles of Science.* (London, 1887.)
[2] W. R. Boyce Gibson, *The Problem of Logic.* (London, 1908.)

Let us now look at the crucial difficulty raised by the classical formulation of inductive inference. The latter has three components : (1) A finite set of instances or data ; (2) that which is called the " conclusion " or generalization and which consists in the enunciation of what is often referred to as a " general law " ; (3) the inference, consisting in transcending, stepping or moving [1] from the first to the second component.

Now the difficulty that arises in connection with this type of inference is as traditional as the formulation itself. Undoubtedly it is said, we possess general laws. Furthermore, a large number of these are obtained from observations.[2] Therefore we must have inference, i.e. a movement from observation to law. And the traditional difficulty is that evidently a gap exists between observations (finite in number, relatively incorrigible, categorical) and law (comprising an infinite number of instances, hypothetical in form.) Prima facie however there is a strong conviction that " valid " inference allows of no gaps.

Is this gap inevitable ? And does every form of inductive logic demand its closure ? Where shall we look for the answer to these questions ? What has been done with this problem by others ? The literature is, of course, vast. Let us look, by way of example, at a celebrated passage in C. A. Mace's " Principles of Logic " [3], a book which fairly characterises the classical tradition. Mace finds himself unable to discover any real basis for the possibility of induction—for the possibility of proving the existence of empirical laws. Significantly however he adds that the fact that scientists have discovered *some* laws constitutes " the most tangible basis of our assumption that induction is somehow valid ". Prima facie, this argument seems to involve a dilemma. Either we have no basis, in which case we cannot *know* that the scientist has ever discovered a single law in the sense required by the argument. Or we *know* that he has discovered some such laws, in which case it is strange to have any doubts as to whether they have a basis. Now the fact that Mace puts forward his argument in all seriousness suggests that he is using the term " law " in two different ways. When a scientist discovers laws he discovers laws in the " pickwickian " sense of that term. When Mace seeks for a rational basis for the possibility of such laws he is *not* using the term in this sense, but

[1] The " movement " may be logical or temporal ; the distinction is not important for our present purposes.

[2] I am not considering high-ranking scientific axioms, *e.g.* Newton's laws of motion, conservation principles, Relativity principle.

[3] C. A. Mace, *The Principles of Logic.* (London, 1933), p. 380.

presumably some other rather more rarefied meaning is involved.

All this suggests that though the difficulties here indicated may have been stimulated initially by a contemplation of scientific methods of enquiry—and we shall do well to pay attention to this factor in the sequel—the real core of these difficulties and particularly any hope of their " solution " will have to lie in a different direction. This should point towards the need for an interpretation of the logical terms or categories involved in the discussion, towards an investigation of the " weight " that logicians have put on these terms. Thus in our example we begin by saying that on the one hand we are given a set of facts, observations, limited in number, taken to be veridical but perhaps liable to error, lying in the past and present. On the other hand science " undoubtedly " consists of a number of " laws ", whose instances are said to be infinite in principle and lying in the past, present as well as future. But then, without any further question, this *description* of science is taken over into logic and made the basis for a problem and a conflict. Is it not possible, however, that this description may perhaps *not* necessarily entail such a problem ?

It is usually said that when we *reflect* on the logical procedure of science we come to realize certain difficulties and discover the need for making certain " assumptions " which are to make the procedure possible. I am suggesting that this " reflection " is frequently not so much a reflection in the literal sense of that term but involves an interpretation, an expansion or revaluation, even an importation of metaphysical demands.

Bertrand Russell, also an exponent of the classical tradition, presents in his " Human Knowledge " [1] another illustration of this conflict. On the one hand we are dealing with a set of experienced facts ; on the other we also " know " a number of laws. But laws involve happenings which lie in the future. Therefore we must know something independently of experience. And the conflict presents itself as a search for a feature of the world which will guarantee such extra-experiential knowledge ; not finding it only shows that we are somewhat ignorant, merely human. But again we may ask : in what sense do we *know* laws that go beyond the premisses on which they are based ? And this question reminds us at once that Russell's puzzle may be due to a special " metaphysical " reinterpretation of such concepts as " knowledge ", " law " and " inference ".

Nothing will help us more to see what is involved than a study

[1] B. Russell, *Human Knowledge.* (London, 1948.)

of the manner and method of various writers who have attempted to answer the difficulties created by the " classical " formulation of induction as mentioned above. Let me try and paint graphically a picture of the logical situation as it is presented by this formulation. We go from something less to something more. We reach as it were from a solid earthly foundation of fact to a " cloud " situated in the sky (or in the heavens : the analogy of the situation with others encountered in similar ethical, aesthetical and theological difficulties should not be overlooked.) The " solid foundation " consists of singular facts, as represented by such observation statements as " The hydrogen-oxygen mixture in this glass vessel when ignited changed into water vapour ". The conclusion or law based on these statements eventually becomes " The combustion of hydrogen in oxygen always results in water-vapour ". This statement, being the expression of a general law, can be used to predict the outcome of future similar situations. Thus we have on the one hand an observed particular relational situation, on the other a general one, which has been inferred from the former. W. Kneale, in a recent publication [1], remarks on such situations as follows : " One of the most striking characteristics of induction used in natural science is that it goes *in some sense* beyond its premisses which are singular facts of experience ". (Italics mine.) To Kneale this fact that induction *in some sense* goes beyond the premisses is serious because it is hard to square with his interpretation of induction as " *rational* inference from observed to unobserved matters of fact . . . lying in the future ". (Italics mine.) The inference must be " rational ". It must be, as another writer [2] puts it, validly carried out. I understand this to mean that the movement from premisses to conclusion must be in some sense continuous and allow of no gaps. And this implies, I think, that " rational inference " is interpreted as an inference, which, from allegedly true premisses, *must* lead to true conclusions, or must lead to them with a definite mathematical probability. On the other hand, the form of the traditional inductive argument seems to demand a " gap " and this prima facie always involves the possibility of a conclusion turning out to be false.

Once more it will be noticed that the conflict which arises here appears to be based on a certain analysis of such terms as " inference ", " rational ", " knowledge " and so on, although, as

[1] W. Kneale, *Probability and Induction*. (Oxford, 1949), pp. 44-5.
[2] L. S. Stebbing, *A Modern Introduction to Logic*. (London. 1933.)

we shall see, much of this analysis may be suggested by empirical considerations. It is therefore unlikely that empirical evidence alone concerning the constitution of the world will be able to resolve this conflict. (Hume's difficulty has never been resolved by an appeal to facts; and Kant's "solution" emphatically offers us rather an analysis of our modes of thought.)

How then can the gap be bridged? And bridged it must be! For it is a remarkable fact, of deep psychological significance, that those who are most severe in their tests of rational inference labour most strenuously to present us with a solution of their problem. Referring back to our "graphical analogy", the following ways suggest themselves quite naturally.

(A) We may deny that the facts are as earthly as here presented and suggest that they participate of the nature of the cloud.

(B) We may deny that the cloud is situated in the skies.

(C) We may agree that the facts are earthly and the cloud is in the skies, but deny the need for or fact of any connection.

(D) We may, with "natural piety", accept the status of both fact and cloud (as under (C)), yet look for a link, for a sort of "Jacob's ladder".

The doctrines here illustrated by means of a pictorial analogy have all at various times and places been maintained by different schools of writers. Thus, corresponding to (A) above we have the doctrine that inductive inference is actually only concerned with inference from generals to generals. Corresponding to (B) we have that all inference moves from particular to particular or, as a second version, a doctrine which assimilates laws to summaries of particular observations. (C) The third view is that there is no such thing as inductive inference at all, although laws are viewed as hypothetical general propositions and the data are given as particular observation statements. (D) Finally we have the view that accepts inference from particular to general though it is found that in order to preserve the semblance of a "rational inference" we require certain general premisses or presuppositions —in other words, the classical formulation of induction touched on already.

I shall attempt to show that the doctrines (A), (B) and (C) all attempt to overcome the original problem raised by (D). Referring back to the three components of inductive inference solution (A) reinterprets the first component containing the data. When we quoted the example of the hydrogen-oxygen mixture it was clearly assumed that the premisses constituted a set of contingent particular propositions. In other words, the facts as

such referred to by these propositions are usually taken to be "loose and separate"—to borrow an expression from Hume. Out of this arose the problem of stepping from facts taken to be loose and separate to others allegedly not so but, as some put it, necessarily connected and referred to by means of general non-accidental propositions. Now the doctrine (A) denies that the premisses consist of particular contingent propositions.

N. R. Campbell, in *Physics*: *The Elements*,[1] defends some such view. Though it amounts to nothing more nor less than a decision to reinterpret the logical nature of certain propositions, significantly he supports his arguments by an appeal to scientific practice. What method do we use when stepping from observation to conclusion in a situation such as described by our example? In the first instance, we do not simply *repeat* the instances a number of times; but rather, if there is any repetition its value lies in enabling us to *vary* one or the other factor in the situation, so as to discover what is relevant to the result of the experiment and what is not. Thus, instead of a glass vessel we may use a quartz vessel and finding that this does not alter the results of the experiment we conclude that the material from which the vessel is made is insignificant. This is the sort of conclusion at which we also arrive by the use of Mill's canons. Now it is Campbell's contention that the facts described by the instantial premisses are not "loose and separate", at least: are never *taken to be so*. Instantial propositions are concealed general propositions, equivalent in logical status to that of general laws. Scientific inference, therefore, consists merely in taking a set of general propositions, and eliminating from among them that which is irrelevant. For the procedure could not yield a general conclusion (according to Campbell), unless it started with general premisses. To summarize Campbell's argument: Inference can only be from generals to generals.

A traditional logician would of course point out that Campbell merely removed a difficulty by not mentioning it. For how do we get the general instantial propositions in the first place? Furthermore they would insist that inductive inference must go from particular to general and that Campbell simply has not discussed this.

I do not wish to discuss these objections nor the original argument that gave rise to them. It will be sufficient for our purpose to point out two things. Campbell's solution of the classical problem of inference amounts, in the end, to no more

[1] N. R. Campbell, *Physics*: *The Elements*. (Cambridge, 1920), Ch. 4.

than maintaining that instantial propositions, for all appearances to the contrary, must be "taken to be" general propositions. That is to say, he proposes a different analysis of logical terms. And the classical objection refuses to accept this proposal and insists on a formulation which we know already to involve impossible difficulties.

(B) The second solution broadly denies that inductive inference ever leads to general laws. In its first version it was at times maintained by Mill who insisted in his discussion of the Syllogism that all inference is from particular to particular. Here we clash with the traditional demand that there must be some sort of "expansion" in inference. And when that demand has been made it is added that Mill does not supply the "general premiss" to permit this expansion to take place! How far such an attitude is consistent with the demand for "expansion" it is difficult to say. As a matter of fact, Mill does invoke such a premiss, *viz.* the Uniformity of Nature. This aspect of his doctrine we shall have to discuss in a later section.

In the second version of this type of solution it is admitted that inductive inference is concerned with inference to *general* laws. The writers here concerned, however, maintain that these laws do not in fact refer to an infinite set of instances but are of a purely descriptive nature. Such laws are nothing but descriptions of the result of past observations, mere summaries, and in no way go beyond these. On such a view also no gap would arise between data and laws since the laws merely summarize the data. Let us call this view the "descriptionist view". Objections to it will be taken under (D) below.

(C) This argument, unlike the first, agrees that what we call evidence for a scientific law does deal with particular, *i.e.* non-general propositions. And unlike the second argument it also holds that scientific laws do go beyond the evidence in the sense that they comprise an infinite series of instances and have the form of hypothetical general propositions. In fact, they always characterize a slightly idealized state of affairs (corresponding to the "smoothing" of a mathematical curve, for instance). This view, however, attempts to grapple with the problem of transcendence by denying the existence of the third of the components of inductive inference mentioned above. In other words it denies that there is any *inference* from evidence *to* law. For, so it holds in one of its versions, often put forward by logico-positivist writers, these so-called laws are not true propositions but rather propositional functions, or better, procedural rules, in accordance with which we may on occasions,

given certain specified conditions, form propositions describing a specific state of affairs.[1] Observations may suggest a certain generalization but we do not as it were *move* to the generalization. We somehow merely formulate an invariant expression which when adopted may on given occasions lead to the desired result. Procedural rules do not predict: they are useful.

It will be noticed that this view does again not so much deny the fact of inference but rather chooses to describe it in a different and more guarded language. Instead of saying " we *infer*, *i.e.* move from certain propositions to others ", it says " we *construct* on the basis of certain propositions certain others (propositional forms) and then *compare* with future facts ". The procedure of inference is *re-formulated* without changing its nature too vitally, lest others should claim that inductive inference was not being discussed at all. (cf. objection to (A) above). At the same time the view is again made more plausible by highlighting certain aspects of scientific procedure. For the present view has its origins in physics as well as logic. Thus there are to be found in physics undoubtedly expressions which do not directly describe, nor are directly inferred from any observed state of affairs (cf. the circle-hypothesis in early astronomical theory; the first law of motion etc.). Such expressions are largely used as axioms in the construction of deductive systems. All the theorems of such a system are logically related to one another, and it is then a question of fact whether such a system corresponds to an actual state of affairs. It is a most natural way of *speaking* when we say that we do not *infer* such a system or its axioms from the facts but that we test the system for its correspondence with the facts.

At the same time I think it is somewhat questionable to treat all the laws of science on this basis. Undoubtedly the top axioms of a science are not simply inferred from the facts, since they function as axioms. But the latter lead deductively to theorems which before their inclusion in a system are frequently discovered as ordinary empirical laws, which, so the *scientist* thinks, are *inferred* from the facts. (We may compare for instance the hypotheses of Dalton's Atomic theory with the chemical laws

[1] Thus Blumberg writes: " Laws in the descriptive sciences are not propositions but models for building propositions. A law or hypothesis, therefore, is confirmed not verified . . . it is useful in so far as it enables us to predict, *i.e.* in so far as the propositions constructed upon it as a model turn out to be true ". Article in *Monist*, October, 1932, quoted in J. R. Weinberg: *An Examination of Logical Positivism.* (London, 1936), pp. 145-6.

of constant proportion, multiple proportion, etc.). These considerations show the logician to have gone further than the scientist, to have extended the latter's mode of description of *certain* situations to cover *all* situations in which the inductive problem might conceivably arise. They indicate that the present position has its roots in a logical complaint. And of course this complaint is that if the laws of science state necessary connections, are hypothetical in form and so on, then clearly no inferences from matters of fact can ever *lead up* to them. Therefore the Logico-positivist proposes not to *speak* of " inference " but of " construction ", " testing " and so on. We observe once again that logical difficulties are solved by recasting the language in which they at first arose.

Again I will not try and "refute" the logico-positivist view. It might be easy to point out that the so-called " rules of procedure " used in science are peculiar in that they differ from such rules as those used in chess. The doctrine we are here discussing does not *want* to say that these rules are rules of a peculiar nature for it would soon end up by having to say that they were rules about the invariable behaviour of nature ; and this would lead it back to the classical camp.

Similar remarks may be made concerning the doctrines of certain writers who in criticizing the classical formulation of induction object that scientific validation of a law does not consist in going from a number of instances to a more general conclusion (whether the instances be all exactly alike or not), but in the attempt to confirm or refute an hypothesis. (Once more we notice a reformulation of the *language* of induction). This view of induction as inverse-deduction is at least as old as Whewell and Jevons. It has been adopted recently by certain logicians (*e.g.* Professor Popper) as the view that we confirm laws by trying to disconfirm them. This is again suggested by certain aspects of scientific practice (testing of hypotheses). It also constitutes a determined attempt to remove the difficulties connected with the classical formulation of induction. For the present view looks like saying : we never infer, *i.e.* move from observation to generalization. We only *test* suggestions. Such a formulation seeks to convey the impression that induction is not a process in which we somehow miraculously step to new knowledge, in which a number of confirmatory instances somehow enable us to lead up to a general law. The present doctrine is much more careful. Confirmatory instances, so it tells us, are only useful in so far as they—by varying surrounding conditions —assure us that a suggested law is not disconfirmed in any of the

tested instances. Broadly speaking then the present doctrine replaces the language of "inferring a law" by that of "failing to disconfirm an hypothesis". The doctrine thus overcomes the classical difficulty once more by a linguistic reformulation. But does it overcome it? For it also holds that we *confirm* a law by failing to disconfirm it. And "confirmation" usually implies *some* reference to the future. Else "I have confirmed this law" would mean no more than "I have tested this law successfully", which is absurd, since it makes sense to say "Our successful test confirms the law"; and this says more than "Our successful test tests the law". This doctrine then would come to grips with our problem only in so far as it allows the scope of "confirmation" to extend no further than the tests for it. Like our second solution it "closes the gap" by never opening it. And it manages to do this by an interesting adaptation of the language which describes *some* scientific situations.

My observations, then, constitute an attempt to show that the solution of inductive puzzles proceeds by means of an interpretation of certain logical terms, in part by drawing on certain bona fide descriptions of scientific procedure which you may or may not accept. This suggests that perhaps the difficulty which I have called the traditional difficulty of the classical view of inference may be disposed of in a similar way, whilst yet retaining the classical formulation and its virtues.

(D) We are now left with the vast majority of traditional logicians who wish to hold fast to an interpretation which does justice to many details of the logical situation and which insists that the data of inference are particular, that the conclusion is quite unrestrictedly general and that *yet* at the same time there is some such thing as *valid* inference.

We can introduce this view best perhaps by confronting it with the descriptionist and logico-positivist doctrines already discussed under (B) and (C). The methods used in answering those views will underline the thesis that the solution of our problem lies in an awareness of the analysis of the language used to discuss the components of inference.

The traditional objection to the "descriptionist" view runs somewhat as follows. We clearly do not use (or *intend* to use) the term "law" in the sense of "description or summary of past occurrences or regularities". We believe it to have (at least we use it as if it had) *predictive* power. Therefore we must *mean* more by law than either summary or description. Thus Kneale [1]

[1] *op. cit.*, pp. 75, 77.

again, says: " . . . laws of nature are normally expressed in the timeless present, and are *assumed* to be concerned not only with actual instances of some kind but with anything which might have satisfied a certain description ". (Italics mine.) Later again, when answering the logico-positivist view he says " When they have formulated a law of nature, scientists *consider themselves entitled*, . . . to make predictions. . . ." (My italics).[1] Quite apart from the general content of the passages here quoted, I think that the words which I have stressed quite clearly bring out the fact that the way by which the logician proceeds to analyse the " logical grammar " or use of certain central logical terms consists in a certain selective interpretation of the scientific situation; it consists, as it were, in the adoption of hypotheses concerning the use to which scientists put their terms. It is very easy to slide from " By law *we mean* so and so " through " laws *are made to function* in such and such a way " into " laws *are* employed for predictive purposes ". Moreover we imply now that they *can* be so employed and begin to suspect that there must be some feature of the world corresponding to this fact. Kneale, in the course of a similar sliding argument ends by saying for instance, that natural laws *are* " principles of necessitation ". (We will call this the necessitarian view.) All this is interesting because those who had originally maintained that laws *are* merely descriptive, do not in the least deny that they are also *employed* for predictive purposes, or as " principles of necessitation ". They thus merely disagree about the " ultimate " analysis of the term. They would probably not even deny, if pressed, that the grammar of the term " law " does frequently *not* function in the way *they* suggest.

It will be noticed that the descriptionist agrees with the necessitarian that it is a problem for the scientist to make sure that his " descriptions " can be made to work by predicting successfully. But anticipating the difficulties that await him if he incorporates that fact in the use of his logical terminology, he prefers to give a safer, less committal analysis of scientific law, one that will cut short any of the traditional difficulties. The necessitarian complains that this analysis is " incomplete " and offers another. In doing so, I think he offers rather too much, so much indeed that the resulting problems become an embarrassment—an embarrassment which is made to appear inherent in the nature of the universe but which, as we now see,

[1] I do not wish to suggest, of course, that Kneale's ultimate solution of the problem with which he is dealing is traditionalist. I merely quote him for the clarity of his exposition of the classical view.

may as well be due to *his* decisions concerning the correct uses of logical terminology.

For the necessitarian's analysis of "law" pitches the meaning of this term into such a rarefied atmosphere that scientific methods will be quite unavailable in the attempt to prove their existence. Thus we are told[1] that scientific generalizations, scientific laws, contain a connection which constitutes an "irreducible ontological category". This notion cannot be analysed in terms of other natural categories. For the connections asserted by a law, so we are told, are not only a matter of what takes place but of what *would* take place if certain conditions *were* fulfilled. Kneale, once more, is instructive. The passage last quoted from his text goes on as follows:

> "... scientists consider themselves entitled, not only to make predictions about the unobserved, but also, ... to assert unfulfilled hypothetical propositions implied by the law."

But then: "... an unfulfilled hypothetical proposition ... is not a mere matter of fact".

But no proposition of this kind can be derived from propositions which are concerned only with the actual, with actual tests. And that means again that no empirical associations of facts of any kind can ever constitute sufficient evidence for such a connection. On the other hand the existence of such connections cannot be inferred from an observation of the facts either and thus must be due in turn to an analysis of the meaning of scientific law, and of the way in which logicians *intend* to use that term.

The embarrassment of the traditional logician to which we referred above leads to the inability to provide a satisfactory account of the third of the components of inference—that which links premisses and conclusion. In the syllogism in Barbara, for instance, the link consists in the Middle term, or in the relations expressed in the two universal premisses (together with the rules of inference) that bind together the subject and the predicate of the conclusion. Traditional analysis, as we have pointed out, finds it difficult to provide such a link for the case of inductive inference, and the various analyses offered for the other two components (data and laws), and which we have discussed under (A) to (C), were largely determined by the desire to overcome this difficulty of not being able to point to a connecting link. If we wish to retain the traditional analysis

[1] R. M. Chisholm, *The Contrary-to-Fact Conditional* (MIND, Vol. LV. No. 220), p. 306.

the problem becomes acute. Thus Professor Broad once said that the " problem of induction " was a " perpetual scandal ". Stebbing and Mace profess themselves unable to " justify " inductive inference conclusively, except by an appeal to " practice ".

Inductive inference is material inference. Moreover, as we have seen, the traditional analysis of the premisses and conclusion of such an inference appear often as assertions concerning the constitution of things (cf. the phrase " irreducible ontological category ", quoted above). It is therefore tempting to think that if you want to " move " from data to law you must provide a real bridge, you must provide a further fact about the world which shall guarantee this passage. (As I have suggested, with the traditional analysis only a kind of " Jacob's Ladder " would meet the case.) But the traditional logicians are unable to provide such a fact. Of course, a large number of principles, or presuppositions indicating what sort of facts would be required have been formulated, for instance by such writers as Mill, Russell and Keynes. One of the simplest is Russell's " Inductive Principle ", put forward in 1913.[1] Reduced to bare essentials it runs as follows. The more often *a* and *b* are found together (provided there are no contrary instances) the more probable that they form a necessary connection. And further : A sufficient number of cases of association of *a* and *b* will make it nearly certain that *a*-*b* is a law. It is a commonplace that such a principle cannot be shown, either deductively or inductively, to be true or false. What is more serious is that it is maintained that without it no empirical evidence in favour of a generalization (or an hypothesis) can have the slightest tendency to go toward increasing its strength. And yet, as just mentioned, the principle cannot from the very nature of the case be shown to be true.

Now all this leads to a kind of scepticism toward the sciences which in turn strikes the common man as being slightly absurd. It exhibits science as a sort of second best. And yet, as will by now be fairly evident, the principle is merely a conversion of the negative complaint arising out of a peculiarly stubborn analysis of the components of inference into a positive assertion (demand).

Of course, this philosophical agnosticism has its advantages. It makes one careful when putting evidence because the impression is conveyed that it is extremely difficult [2] to move to a

[1] B. Russell, *Problems of Philosophy*. (Oxford, 1913), Ch. 4.

[2] Notice that if we said " impossible " instead of " extremely difficult " the sting would be taken out of the doctrine.

conclusion on the basis of inductive evidence. When you say: The truth of an hypothesis is a matter of the tests which confirm it, you might feel quite satisfied with this or that test, not realising that what was meant was, first, a set of tests carried out by means of a complex specified procedure; secondly, that it was a matter of possibly an indefinite number of such tests depending upon the complexity of the situation. (I am ignoring here of course both statistical considerations as well as the matter of systematization and theory which enables us to employ "crucial" tests. Such considerations are logically secondary to our problem.) The traditional analysis, speaking in terms of inference produces caution. This incidentally makes it sound so sensible.

And there is of course a perfectly good empirical analogy which has served as the basis for Russell's principle. (In fact to each and any of these principles there corresponds some empirical analogy, some suggestion adopted from scientific method.) For quite apart from statistical method, undoubtedly the scientist argues that the more often *a* and *b* are found together (given certain scientific techniques, and other things being equal), the more a law of the form *a-b* becomes acceptable to him. But this empirical or working assertion must not be confused with Russell's principle. For the latter, as it were, represents a logical or even quasi-physical link, a fact about the world, which if you could only know it to be a fact, might enable you to argue deductively to a conclusion. It poses as an assumption which, were it satisfied, would act as a sort of insurance. It functions, in fact, as follows: *a* and *b* have been found frequently associated. And we want to move to: *therefore a-b* is a law. Corresponding to this "therefore" we require *something*, some further fact, law or principle. And the virtue of Russell's formulation of such a principle is that it shows that it is just no more than the assertion that you *may* argue from "the association of *a* and *b*" to the "law *a-b*". And why should we require this special sanction? Surely only, because, as we have seen, the analysis of premiss and conclusion has been formulated in such a way that nothing that you ever discover about the working processes of science, which often *seem to enable* you to come to a conclusion, in the *normal* use of that term, would have any bearing on the way in which you reach that conclusion in the *special* sense of the term.

It is a peculiar fact that most scientists are never aware that they are making assumptions of the kind proposed by Russell and others. This may be due to dull-wittedness, or lack of logical acumen, or ignorance of philosophical principles. But could it not be also due to something else? And I suggest that

the scientific interpretation of "law" and "inference" or "reason-giving" are different. Russell's principle—the principle of "the more, the better"—does not appear to the scientist to be an assumption about the universe. If it means anything to him, it describes a procedure in accordance with which he *decides* whether to put more or less weight on a law with which he is concerned. He does not feel that he requires a guaranteeing principle, dictated by a logician's analysis of law, but rather makes his method of procedure dictate his interpretation of law and inference. In fact, he would be in serious difficulty if his method of procedure were used as a guaranteeing principle. For Russell's principle says: on certain occasions you may go ahead with impunity. But if this were ever possible it would have serious consequences for scientific logic. For it is a commonplace of that logic that scientific laws must always be in principle corrigible. Any principle such as Russell's if it *did* apply should sometimes give us incorrigible knowledge—and we do not want this. Actually, the scientifically-minded person wants two, perhaps contradictory, things. He is always on the lookout for laws which as such will cover an infinite number of instances—and that does make it appear as if he was looking for incorrigibility. But on the other hand he also wants corrigibility. And this must cause him to reject any guarantee of "absolute certainty".

Let us consider an example. Joule's lifework as a scientist consisted in an attempt to *demonstrate* (as he put it) the first law of Thermodynamics, *viz.* that a definite amount of work is always equivalent to a definite and fixed quantity of heat. It is of interest to mention his procedure. He rigs up an experiment which measures the amount of heat produced by a certain amount of mechanical work. He then alters the circumstances of this experiment as a result of which he gets a value similar though not quite identical with the first. This was in 1849. As a result of these two or three experiments he has no qualms at all in passing to a conclusion.

> "I will therefore . . . consider . . . it as demonstrated by experiments contained in this paper that . . . the quantity of heat . . . is always proportional to a fixed amount of work done."[1]

Six years earlier, after the very first experiment of this series, he had already said—

> "I shall lose no time in repeating and extending these

[1] J. P. Joule, Quoted in Magie: *A Source Book in Physics.* (New York, 1935.)

experiments, being satisfied that the grand agents of nature are, by the Creator's fiat, indestructible; and that wherever mechanical force is expended, an exact equivalent of heat is always obtained".

He proceeds over a number of years right down to 1878 (another thirty years) to devise experiments to test this conclusion but he has never any doubt, either about it as such, nor—what is more important—that the further values he obtains tend to *support* his earlier conclusion. Now it may be answered that this was due to certain preconceptions concerning conservation principles. I am not certain that it was but if so they were physical preconceptions and not logical ones. Why should he be so certain then that his experimental results *do* furnish evidence for a *conclusion?* I think it is because "discovering" or "testing" or "confirming" a law functions differently for him than it does for the traditional logicians. To be sure, a law is to him not just a descriptive summary. Rather he *imputes* to it predictive power—as Kneale says. But he only imputes predictive power to a conclusion provided it is arrived at in accordance with certain accredited scientific procedures. If the law turns out to predict falsely under certain circumstances, whilst this would be surprising and perhaps even cause him to amend his procedure, he regards the fact as logically possible. But "he regards it as possible" must not be interpreted to entail: "therefore no matter what he does, his experiments have no tendency to strengthen the presumption that he has here got a law".

Now this interpretation has the following advantage over that of Russell. Nothing that a scientist does and no successful tests of any kind have the slightest tendency towards increasing the likelihood of Russell's assumption (and therefore of the "laws" based on it) since this assumption is a presupposition prior to which no evidence can be accepted. But this runs directly counter to the scientist's experience that a procedure is sensible when it attaches more weight to good than to bad evidence. His use of "law" functions in such a way that evidence *does* go toward strengthening a law without his having to *assume* logically and antecedently that to his *procedures* (*e.g.* that of attaching weight to large numbers) there corresponds anything in the world of the kind suggested by Russell.

To put it in plain language, if his procedure works he will discover this. Someone might now ask (with rationalist preconceptions in mind), how we *know* that this procedure *works*. (Remember the strictures placed on Mace's argument above.)

The reply is fairly obvious : here " works " means what is *normally* meant by this word and what the scientists mean by it. It does *not* mean : having a guarantee that I cannot go wrong.

More seriously, it may be objected that we have merely substituted another principle, very similar to that of Russell, running roughly as follows : A scientific conclusion may be inferred provided it is based on evidence collected in accordance with a certain procedure. This would however misconstrue the thesis of this paper. I do not put forward a principle to be invoked as a major premiss. On the contrary, I contend that no such principle is nor can be required to close the gap between premiss and conclusion provided we avoid any interpretation of the components of inference that would necessitate such a principle. In fact, the necessity for it is contingent upon such an interpretation. It is just this fact—having shown that the traditional logician's case does not so much spring from any inherent feature of the world, but from an interpretation and choice of logical grammar, that enables us to employ an *alternative logical grammar,* equally suggested by certain aspects of scientific procedure and eliminating the necessity for a principle—already known beforehand to be unknowable.

I shall now reconsider and summarise. The " scientist " says : " I accept law (L) on the basis of evidence (E), duly collected in accordance with the procedure of my craft ". Thus Born [1] for instance, one of the most brilliant contemporary physicists, says :

> " . . . while everyday life has no definite criterion for the validity of an induction . . . , science has worked out a code, or rule of craft, for its application. This code has been entirely successful, and I think that is the only justification for it. . . . "

The " logician " translates : " Scientist X says L is true " ; from this he infers : " X knows L to be true ". From " knowing L to be true " he then further infers that X knows all the logical consequences of L, and from this again that X knows what will happen in the future, what has not yet been experienced etc. (Compare Russell's difficulty, mentioned above.) It is this " consequence " that the traditional logician finds intolerable. For he sees quite clearly that the evidential instances do not carry labels certifying the future—a fact pointed out by Hume long ago. Paradoxically, it is now seen to be impossible for the " evidence "

[1] M. Born, *Natural Philosophy of Cause and Chance.* (Oxford, 1949) pp. 6-7.

to function as evidence for a conclusion at all; indeed, the inference can never as much as begin. He now *describes* this paradoxical situation by a reference to the need for special guarantees ("Uniformity of Nature", "Simplicity of Natural Laws", "Limited Variability" etc.) He may even go so far as to read such guarantees into nature—compare Mace, above: the fact that we have discovered some laws constitutes a basis for induction; again Mill: We can discover the "Law of Causation" by induction. But of course, the doubt persists as a "perpetual scandal" and is incurable. Moreover, it persists in the face of the scientist's belief in the correctness of his conclusions. This is due to the fact that for the scientist "knowing L to be true" functions differently and roughly as follows: (*a*) This is a respectable law, *i.e.* has been arrived at in accordance with accepted procedure; (*b*) I *assert* that this law *will* also yield fruitful results; (*c*) however, "I know L to be true" does *not* function so as to imply that I know *in a special way* that it must yield such results, although I *do* know that such laws yield fruitful results more often than not and can suggest reasons for this (in the technical sense).

This is not to say that some scientists are not themselves affected by the traditional analysis. Indeed, Born himself, just previously to the passage cited above tells us that

> "... it looks as if science has a methodical way of finding causal relations without referring to any metaphysical principle. But this is a deception. For no observation or experiment, however extended, can give more than a finite number of repetitions, and the statement of a law ... always transcends experience."

Usually, instead of proceeding to the demand for a guaranteeing major premiss, such scientists say that the conclusions of science are merely doubtful or probable. Of course, doubtful they are, in the *empirical* sense. Scientific statements often happen to be corrigible. The present type of doubt, however, is clearly not of this order though again suggested by it. For the "probability" is never calculated and clearly infests *all* scientific statements equally, whatever their "empirical" validity.

We can then only conclude that for scientists *not* affected by the traditional analysis statements such as "I accept L on the basis of E ..." function differently, do not have the consequences and do not allow of the translations given by the traditional logicians. And more generally it appears that a treatment of the "puzzle of induction" hinges upon a decision as to which features of scientific procedure and terminology are to be used

in the choice of a suitable logical grammar and not upon an investigation of nature as studied by that scientific procedure itself.

The substitution of a different logical grammar is a perfectly permissible method for "curing" this puzzle. For we have, I think, shown that *all* interpretations of the "nature of inductive inference" are decisions concerning the use of the terminology of inference.

We see then that throughout the history of this problem certain difficulties have been approached by means of putting forward logical analyses of the concepts involved. Sometimes the analyses are so constructed as to suggest an immediate solution. Sometimes they are made so severe as to preclude the very possibility of one. Despite this fact, even in the last case a solution is proposed, albeit of a "non-natural" type: either "complete scepticism", or alternatively a "belief in Uniformity". The latter is used as a kind of "magical mirror" and held up to the "traditional complaint"—as we have called it. But on closer inspection the mirror turns out to be just an ordinary one. Instead of showing us the image of a special sort of fact (a supra-natural fact), it's just a mirror reflecting—as it were—the complaint itself.

The original "empirical" difficulties (for instance doubts connected with the complexity of nature), though having been illuminated have yet necessarily been left untouched. There is no harm in this for we knew all the time that the original trouble could only be dealt with by the methods of practical science and enquiry. At the same time it is however important to remember that decisions about logical terminology and constructions based upon them, whilst illuminating the intentions of logicians and scientists, fail to make contact with the realm of empirical fact.

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III.—NATURAL CHANGE IN HERACLITUS

By G. S. Kirk

The thought of Heraclitus of Ephesus is still often summarized as "All things are flowing", *πάντα ῥεῖ*; by which it is inferred that everything is in *constant* change. This summary goes back ultimately to Plato, who at *Cratylus*, 402a, wrote as follows: "Heraclitus says somewhere that everything is moving and nothing stays still, and likening things to the flow of a river he says that you could not step twice into the same river". Plato's interpretation was adopted by Aristotle, and through him by Theophrastus, whose "Opinions of the Physicists" became the basis of all later ancient accounts. Recently, however, some scholars have become sceptical about the accuracy of the Platonic-Aristotelian interpretation of Heraclitus' views on change; and with good cause, for the fact is that there is nothing in the extant fragments about the constant flux of all things, even though one would have expected the survival of some original support for a view so widely popularized in the fourth century. The assumption from this is that the constancy of change is not an idea which Heraclitus particularly stressed. What he undoubtedly did stress above all else was his discovery of the unity that subsists in apparent opposites: it is with failure to apprehend this unity that he so bitterly reproaches his fellow men. Plato bears witness to this theory as well as to the theory of change, and Aristotle mentions it repeatedly because he thought that Heraclitus was thereby denying the law of contradiction—which shows how little Aristotle appreciated the real application of Heraclitus' *grande idée*. Later, Philo asserted categorically that Heraclitus' vaunted discovery was simply that if a unity is split opposites are revealed, and that opposites are really one; a discovery, Philo typically adds, which should really be credited to Moses.

How is it then, we may pertinently ask, that Plato gave such prominence to the idea of constant and universal change in Heraclitus? Fortunately we possess a pair of certainly genuine fragments which are in themselves capable of having misled Plato, who, it should be remembered, did not set out to be a historian of philosophy, and who never took Heraclitus quite

seriously in the dialogues. The more important of these fragments, 12 in Diels' order, says : " Upon those who step into the same rivers, different and different waters flow " (*ποταμοῖσι τοῖσιν αὐτοῖσιν ἐμβαίνουσιν ἕτερα καὶ ἕτερα ὕδατα ἐπιρρεῖ*). The following sentence, " and souls too are exhaled from moisture ", must be counted an irrelevant addition by the Stoic Cleanthes, to whom ultimately the preservation of the fragment is due. The second fragment, 91, consists simply of three pairs of verbs describing water in a river : " Scatters-gathers : concentrates-disperses ; approaches-departs " (*σκίδνησι καὶ . . συνάγει . . συνίσταται καὶ ἀπολείπει καὶ πρόσεισι καὶ ἄπεισι*)—the rest being merely interpretation on Platonic lines by Plutarch.[1]

Now Karl Reinhardt has shown [2] that Heraclitus says nothing here about *things being like* a river, but merely points to a certain aspect of the behaviour of rivers in general. Some think that fr. 12 is no more than another example, purely formal in type, of the coincidence of opposites—in this case of " same " and " different " : upon those who step into the *same* rivers *different* waters flow. But this is to reduce its emphasis too much, and there are serious objections against this interpretation : first, the other Heraclitean examples of the coincidence of opposites are far more concrete, less purely logical, than " same-other " ; witness " summer-winter ", " war-peace ", " the young-the old ", " the straight-the crooked ", " the way up-the way down "—for Heraclitus, these were not abstractions as they are for us. Secondly, such examples in other fragments are unmistakeably framed as such : " the way up and the way down is one and the same ", and so on. Thirdly, the identification of " same " and " other " would destroy all differentiation, while Heraclitus was content that his unity should be an underlying one, an *ἀφανὴς ἁρμονία* : he was not Parmenides. What these river-fragments *are* intended to show, I believe, is the regularity, the order, the *μέτρον* or measure, which Heraclitus believed to underly and to control natural change in all its forms. The example of the river is intended to illustrate this *μέτρον*. The repetition of the word " different ", *ἕτερα καὶ ἕτερα*, well suggests the regularity of the onrush of waters, although it is no

[1] The other commonly-accepted river-fragment, 49a, consists of a later paraphrase of fr. 12 to which the un-Heraclitean deduction " we are and are not " has been added. It has no value as evidence for Heraclitus.

[2] Most clearly in *Hermes*, 77 (1942), 18 f. ; see also his *Parmenides und die Geschichte der griechischen Philosophie*, 177, where he touches briefly on the implications of the river-image which are stressed below. He does not, however, face the difficulty caused by his supposition that fr. 12 comes from a psychological context.

more than a suggestion ; and the oppositions of fr. 91, which in its original setting may have followed directly upon fr. 12, express the reciprocity and quantitative balance much more unmistakeably : " it scatters and gathers, concentrates and disperses, approaches and departs ". The continued existence of the river as a whole, of the " same " river in Heraclitus' terms, depends upon the maintenance of this regularity in the movement of the waters past a fixed point, the ἐμβαίνοντες. Of course the idea of the preservation of a kind of stability in change is there too, but it is subordinated to and dependent on the idea of μέτρον. The river-fragments, then, seem to exemplify not the constancy of change—for there is no hint that all things resemble rivers—but the regularity of natural change in one particular manifestation.

Before this interpretation can be accepted (and it is pitted against that of Plato himself, a powerful authority, though I suspect that he may not have known as many of the actual sayings of Heraclitus as even we do) it must be compared with the evidence of the other extant fragments. Does the idea of measure in change appear prominently there ? Indeed it does : in fact once the idea of μέτρον is isolated it can be seen springing up everywhere. This of course is the trouble with Heraclitus ; any idea which arouses the student's enthusiasm can do the same —that is why we have Heraclitus the Hegelian and Heraclitus the Existentialist. However, consider the evidence. In fr. 30 the cosmos is an everliving fire, kindling *in measures* and going out in measures (ἁπτόμενον μέτρα καὶ ἀποσβεννύμενον μέτρα). In fr. 31 the sea is measured (μετρέεται) into the same proportion as applied to it before it became earth. In fr. 90 fire is an exchange for all things and all things for fire as goods for gold and gold for goods. In fr. 88 (of a group of opposites like summer-winter) " these things change places and are those, and those change places again and are these ", where μεταπεσόντα implies a regular exchange. In fr. 94 " the sun will not overstep his measures (μέτρα) ; if he does, the Erinyes, agents of Dike, will find him out ". In fr. 51 that which tends apart also coincides : it is a παλίντονος ἁρμονίη, a join which works in both directions like the string of a bow or a lyre ; note that here too the tension must operate equally in each direction—the inward pull of the string must equal the outward pull of the arms of the instrument, otherwise the string is too loose or the whole instrument breaks. Akin to the idea of measure is that of plan and direction in the world, as in fr. 41 : wisdom is to know how all things are guided ; and in fr. 80 : all things happen by strife and *necessity*. The

concept of Logos supplements this whole picture; (Logos for Heraclitus is the single formula or plan according to which all things happen (fr. 1), . . *γινομένων πάντων κατὰ τὸν λόγον τόνδε*); so also the use of *κόσμος* in fr. 30: "This *κόσμον* no man or god made; it was, is, and shall be". Now *κόσμος* for Heraclitus, in the early fifth century, must still have retained much of its basic meaning of "order", "regularity"; it cannot just mean "world" in our practical sense, and is perhaps best translated as "organism".

Two related questions may now be asked. First, if the river-fragments do not contain the idea of universal and constant change, how far was this idea held by Heraclitus? And secondly, why such insistence on *μέτρον*? The answer to the first question is that the universality of change, though not its absolute constancy, was a commonplace of early Greek thought which Heraclitus cannot have avoided: change is going on everywhere, you only have to use your eyes. The Milesians did not think it necessary to give a formal explanation; probably they regarded all things as alive, and change is a property of life. Heraclitus had to be a little more explicit, because the unity which for him connected all natural existents depended on the inevitability of change, sooner or later, in every division of nature; while the somewhat different unity of the Milesians and their mythological forerunners depended upon a world-forming process out of a single source, a process which gradually slows down and in the final stages of which change is no longer indispensable to unity, but is taken for granted. Heraclitus' unification of apparent opposites depended in its clearest form upon an unfailing reciprocal movement between extremes: night succeeds day and day night, therefore night-day is a single continuum; so too with the other pairs of opposites; therefore, he concluded by an intolerable leap of the imagination, all things are one. If the succession fails the unity is destroyed, and with it the Logos which relates man to his surroundings and is therefore so important to understand. The reciprocity *must* continue, and it does so, says Heraclitus in metaphorical terms which are logically no advance on the reproductive imagery or the automatic assumption of his predecessors, because "war is common and strife is justice", because "war is the father and king of all", because "it rests by changing". It continues because things are an everliving fire and fire is creative, as in animal reproduction, and self-moving. But this is not to say, as will be seen later, that everything is flowing in the sense that it is changing at every instant. The answer to the second question, why there is such emphasis on

μέτρον, is not dissimilar: the unity which subsists in opposites depends not only on their alteration one into the other, but also on the quantitative regularity of this alteration. If the total amount of old age in the world begins greatly to exceed the total amount of youth, then the succession will eventually fail. If the total amount of heat and dryness in summer begins to outweigh the total amount of cold and wetness in winter, or vice versa, first the crops will fail and eventually the earth will be overcome by one of those catastrophes of fire or flood which are so often hinted at in Greek literature, and which belong perhaps to one of the earliest stages of primitive mythology, but which for Heraclitus at any rate were not more than theoretical contingencies. If the balance of processes is destroyed then the underlying unity of the cosmos fails, and this, for Heraclitus, was unthinkable. And this balance depends on μέτρον.

This idea of μέτρον was taken by Heraclitus primarily from the sphere of ethics and applied by him to the workings of nature in general, but most clearly to natural changes on the large scale; though we have seen that it underlies reciprocal change on any scale. For although Heraclitus had broken away from the old cosmogonical tradition, and although judging from the complaints of Theophrastus he did not devote much time to specific natural questions like What is a rainbow or What causes the flooding of the Nile, yet he could not and did not neglect to give some explanation of cosmology, the working of the world which men see around them. Indeed, if as he maintained there is a single Logos or formula of things, this Logos must explain meteorological and cosmological changes as well as the reciprocity between opposites in categories such as life-death and war-peace. Strangely enough, however, we do not find that Heraclitus used his discovery of the unity of opposites, in any obvious way at any rate, to explain cosmological phenomena. The cosmos (in the sense of an ordered whole) is a fire which turns into sea and into earth and then back again. This is a reciprocal and not a cyclical movement, but it is a reciprocal movement between three and not two members—that is, not between opposites—so that the simple logical unity that connects for example night and day does not apply here. Formally the only common factor between Heraclitus' account of meteorological-cosmological change and his account of change between opposites, which is implied to be the type into which all other kinds of change can be analysed, is the idea of μέτρον. In fr. 31 we learn that the turnings of fire (πυρὸς τροπαί) are as follows: first into sea, and half sea is turning to earth, half of it being replenished from fire; the portion that became

earth eventually dissolves again and is measured into the same amount of sea as existed before it became earth. What is described here in these complicated and schematic terms is not, as Theophrastus thought, a world-forming or cosmogonical process; it is the constant weather-process by which the sun feeds on water evaporated from the sea, and precipitates it again as rain; part of the sea is drying up (for example Heraclitus' own Cayster river, which was silting badly; the fossils in Sicily, Paros, Malta, cited by Xenophanes; the legendary emergence from the sea of Rhodes and Delos), while an equal part of it is expanding (for example the submergence of the Strait of Messina, and the rise of new springs and rivers). As long as these large-scale natural processes remain in balance the unity of the cosmos is preserved, and the total amount of fire in all its forms remains the same. When he tried to explain how or why the *μέτρα* were preserved Heraclitus resorted to metaphor, as he did in the case of the source and motive of change. If the sun oversteps his measures[1] the Erinyes, traditional guardians of natural laws, will find him out. This resort to mythology and metaphor occurs at a certain point in all the Presocratic accounts (and indeed in all philosophies) and is exemplified by the "penalty and retribution" of Anaximander and the "strong Necessity" with her fetters of Parmenides. It occurs as an attempt to *motivate* the structure or *μέτρον* which has been observed in or deduced from phenomena.

In this large-scale sphere of meteorological-cosmological change the process is clearly spasmodic; thus all sea is not always being evaporated, and parts of the earth may remain static for a time. If we look lower in the scale we see that there too, in Heraclitus' formulation, no necessity exists for a *constant* change in everything. Man, it is true, is in unceasing change: he is constantly growing older and as he does so the structure of his body alters. Thus a fragment which is ascribed to Epicharmus describes how a debtor excuses himself by saying that he is not the same man as he who incurred the debt, his Logos has changed.[2] But when one descends still lower in the scale, to less animate objects like rocks and tables, it becomes exceedingly doubtful

[1] This probably means—for *ὑπερβήσεται* should be taken literally at this stage of the language—if he trespasses too far north or south on the path of the ecliptic, and so upsets the seasons: *μέτρα* is still quantitative as always in Heraclitus, though in this case it would have the sense of "boundaries". It may just mean quantitative measures of fire, *i.e.* the sun must not become too large and hot.

[2] The same idea, applied again to the human body, recurs in Plato; it is not, I suspect, a specifically Heraclitean invention, but a popular and traditional witticism or figure of speech.

once more whether Heraclitus believed in *constant* change for everything. This would be hardly worth stressing were it not that one school of thought, of which Heidel in his article "Qualitative Change in Pre-Socratic Philosophy" (*Archiv. f. Gesch. der Philosophie*, 19 (1905-06), 350 ff.) was representative, actually proposed a kind of molecular theory according to which this table, for example, is constantly changing by the *invisible* addition and subtraction of portions of fire, water, or earth, on a par somehow with the cosmological process. This unwarranted interpretation seems to stem from Aristotle, who in one passage (*Physics* Θ. 3. 253b9) says, clearly of Heraclitus and his supporters: "And some say that all existing things without exception are in constant movement, but that *this escapes our perception*". It is most unlikely that Heraclitus ever held such a view. Contrary to what is often written of him, he believed strongly in the value of sense-perception providing that it is interpreted intelligently, with φρόνησις, by souls which understand its language.[1] His criticism of men is based on the fact that the truth is there to be observed, it is common to all, but they cannot see it: apprehension of the Logos is no mystical process but the result of using eyes, ears, and common sense. Our observation tells us that this table is *not* changing at every instant, even if our experience concedes that it will eventually change. This eventuality is all that is necessary: just as the movement between war and peace, for example, was inevitable but nevertheless, in Heraclitus' day, spasmodic and not continuous, so objects may be held temporarily in stability by virtue of a παλίντονος ἁρμονίη; the tendencies to turn into earth or fire may be equally balanced. Provided the total μέτρα in the world are preserved a large number of things may and do exist for a time without changing; but eventually the tension in one direction or the other will dominate and the material composing this table will return, perhaps deviously, to the fire from which it was originally extinguished.

The theory that all things are *constantly* changing was perhaps first explicitly formulated not by Heraclitus but as a manifest absurdity by the Eleatic Melissus.[2] He was trying to defend the paradoxical Eleatic idea of reality by attacking the validity of the senses, and wrote as follows: (fr. 8): ". . . But to us the hot seems to become cold and the cold hot, and the hard soft and the soft hard . . . iron in spite of its hardness seems to be

[1] Cf. frr. 55, 107, 101a; 17, 72.

[2] This was suggested by E. Weerts, "Plato und der Heraklitismus", *Philologus*, Supplb. 23, 1 (1931).

rubbed down by the finger through contact, and so also gold and stone and whatever else seems to be hard and fast; and earth and stone seem to come to be out of water; so the consequence is that we neither actually *see*, nor recognise things which *are*". Some of the oppositions here may be taken from Heraclitus, but I believe that the examples of iron, stone, and gold, are an extension by Melissus himself, who had far more motive (though in a negative direction) for emphasising the constancy and universality of natural change than Heraclitus ever had.

For Heraclitus then what we see in individual things is either the prospect or the actuality of quantitatively regulated change. What we see in the sum of things, changing and temporarily stable, is the single Logos which is a broader aspect of the μέτρον which regulates all change. The concept of measure equals in importance, and surpasses in the consistency of its application, that other basic concept of War and Strife, of inevitable alteration. Both concepts are needed to make plausible the kind of unity which Heraclitus saw in opposites; both are needed to mediate between this unity and the other unity of Fire. In the river-fragments the concept of μέτρον in change is the one which is stressed: to ignore this concept and build upon these fragments an anachronistic elaboration of the War-Strife concept, like the Platonic πάντα ῥεῖ interpretation, is to destroy that unified picture of the outside world which Heraclitus tried to present to obtuse mankind.

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IV. OBSERVATIONALISM IN PSYCHOLOGY

By R. Peters

Introductory

It now seems to be quite widely accepted that philosophical theories have a second order character in that they are either descriptions of or recommendations about the procedures of first-order activities like scientific research and moral behaviour. Often, so it seems to me, these procedural reviews have had an effect on the procedures of scientists and moralists. Philosophers, whether wittingly or unwittingly, have contributed to creating or changing rules of procedure in their endeavours to understand them. Most people, with the examples of Kantians and Utilitarians in mind, would concede this point with regard to morality, but they might feel that it was rather far-fetched with regard to science. Surely, they might argue, scientists pay scant attention to the methodologists; they just get on with their job of testing hypotheses without worrying much about what the methodologists say. Now this may be a tenable point of view with regard to a lot of workers in the advanced physical sciences, but there is evidence to suggest that the practice of many social, scientists and psychologists is or has been strongly influenced by certain philosophical assumptions about method. Professor Popper, for instance, in his "Open Society", tries to reveal and combat the influence of "historicism" on social science. He also attacks frequently what he calls "observationalism" or "inductivism". In this article I want to support his general attack on "observationalism" by suggesting that this philosophical description of scientific method has in fact exerted an influence on the development of a particular science, psychology—an influence which has not been altogether fruitful.

1. What is Observationalism?

The view, which Professor Popper refers to as "observationalism", started with Aristotle and became influential after Francis Bacon in the writings of those philosophers of science who tried to provide an alternative to the Cartesian account of the acquisition of knowledge. They tended to share Descartes' rationalism in thinking that there is an infallible method for

acquiring certain or highly probable knowledge but differed radically in their account of the required recipe. They put a salutary stress on observation as opposed to deduction from axioms and substituted for Descartes' simple natures, sensory atoms collected by simply looking at Nature. They maintained not only that scientific laws were descriptions of invariable sequences of these sensory atoms but that things also, including ourselves and others, were clusters of such sense-data built up, as a matter of psychological fact, by correlating such atomic sense-data. This psychological description and the psychology which developed from it, though later abandoned by sense-datum theorists, was a way of re-inforcing the view of scientific method which succeeded that of Descartes. Locke and Hume created a psychology dictated largely by their epistemological pre-occupation. Locke's passive mind, for instance, was a way of assuring us that we could not make mistakes about the simple ideas which provided a solid foundation for knowledge. His pre-occupation with the certainty of knowledge influenced his quest for its " original ". Hume's isolated and incorrigible impressions served a similar epistemological function. Locke and Hume established a tradition both of psychology and philosophy and the psychological tradition was strongly influenced by their philosophical views about the correct way of obtaining knowledge.

The inductive account of scientific method, which is an alternative way of stating observationalism, postulated the careful and meticulous collection of data by " pupils of Nature ", the cautious generalisation which must not go beyond the data, and the " interpretation " which emerged when a judicious man like Francis Bacon surveyed the tables of classified data. This picture of the scientist in action, combined with the Kantian aphorism that a discipline is as scientific as it contains mathematics, led to the tacit acceptance of the view that the scientist proceeds by observing events in Nature, measuring them, noticing correlations or laws between the sets of measurements, and finally relating laws under theories.

There emerged, therefore, two connected presuppositions about science which had a considerable effect on psychology. In the first place it was assumed that a science is to be distinguished by the special kind of " data " or " subject-matter " from which it starts. In the second place it was assumed that a science must start from impeccable " data " *e.g.* measurements—if theories were to emerge at the end. Let us briefly assess these two presuppositions before illustrating their effect on psychology.

Few would deny that emphasis on " data " and on " subject-matter ", in so far as it was a way of stressing the observational basis of science, was a salutary antidote to Cartesianism because it brought out, if in rather a misleading way, that scientific hypotheses are *tested* by observation. The objection to Locke, Bacon, and Hume is that they maintained that the scientist must *start from* observations. This tended to make people neglect the importance of hypotheses in science. Instead, in psychology, for instance, there were endless arguments about what the " subject-matter " of psychology was. No doubt there is a quite harmless and usual sense of " subject-matter " in which, on any account of scientific enquiry, petrologists, ornithologists, and astronomers can be said to have a " subject-matter " of their own. Some scientists single out certain classes of objects and ask questions about rocks rather than rooks or stars. But this rough and ready distinction in terms of objects of interest is not much help in the sciences of man which are all concerned in one way or another with the behaviour of human organisms. For practical and didactic purposes the sciences of man can be sorted out by reference to the sort of questions asked about man and the types of procedures used in answering such questions. This facilitates the organisation of university departments, enables examination syllabuses to be drawn up, and ensures that rabbits are not laid out on the benches of the chemistry laboratory or microscopes in the geography room. But nothing of *theoretical* importance depends upon such divisions. The social scientist or psychologist, who does research as well as teaching, is a student of problems, not of subject-matters. Some of his problems cut across all usual clear-cut distinctions of " subject-matter ". Neuro-physiologists, for instance, are now making discoveries about the pre-frontal lobes which are exceedingly relevant to the kind of problems with which Freud was confronted—the aetiology of changes in social dispositions. A working scientist cannot rule out possible answers to questions because they are not on the syllabus. If, therefore, we find psychologists seriously perturbed about what their " subject-matter " is we may reasonably suspect them of being haunted by the old Baconian picture of the scientist who must start from certain specific and clear-cut data.

Again, on any view of scientific activity, a prominent place must be found for the part played by measurement, the main function of which is to facilitate the testing of hypotheses. Quantitative techniques enable scientists to answer more exactly questions unearthed by cruder qualitative methods. But there

is no need to bring this out in the observationalist manner by saying that the scientist *starts from* measurements or " operations " This methodological rule was certainly not observed by people like Freud and McDougall who made greater contributions to the advance of psychological theory than any of those who became pre-occupied with obtaining quantitative " data ". As Kohler says : " If we wish to imitate the physical sciences we must not imitate them in their contemporary, most developed form. We must imitate them in their historical youth, when their state of development was comparable to our own at the present time . . . let us imitate the physical sciences, but intelligently."[1] Some psychologists, for instance Clark Hull, being wise to the proper function of measurement in the development of explanatory theory, have constructed systems of measurement alongside the development of testable hypotheses.[2] But this has not been the case with all psychologists who have become preoccupied with measurement. Is it not plausible to suggest that much of this premature quantification was a consequence of observationalist presuppositions ?

In this article I shall endeavour to substantiate my case about the influence of observatioaalism on the practice and methodological discussions of psychologists by citing some historical examples. The case of measurement in psychology might provide detailed evidence ; but it would need a separate article for adequate treatment. Similarly a separate article would be needed for the case of the " understanding " psychologists who thought that psychology must be a branch of history rather than science because of the peculiar and unique " data " from which we must start in studying human beings, or for that of some Gestalt psychologists who stressed the importance of starting from " direct experience " (*e.g.* Kohler) or " the behavioural environment " (*e.g.* Koffka). I intend, however, to confine myself to the controversy between behaviourists and introspectionists and to the development of the operationist school in psychology, these two examples being clear illustrations of the influence of observationalism.

In a thesis of this kind there is a great danger of appearing to claim too much. I do nct want to deny for a moment that there were other reasons for the development of these " schools " of psychology which others, like Woodworth,[3] have brought out.

[1] W. Kohler : *Gestalt Psychology*, pp. 31-33.

[2] See C. Hull : *Principles of Behaviour.*

[3] See. R. Woodworth : *Contemporary Schools of Psychology* (Rev. Ed. 1949).

But I do not think that any of those who have treated comparatively the different "schools" of psychology have drawn attention to the common, and in my view mistaken, methodological assumptions without which the almost fanatical fission of psychology into "schools" need never have arisen. "Schools" of psychology do not reflect simply different interests or rival hypotheses. If this were the case they would be a healthy symptom. They reflect, as well, rival recipes about how to do psychology. How weary we get of that introductory chapter to psychology books which maintains either that the enquiry to follow is perfectly scientific or that the enquiry to follow is the only useful sort of psychological enquiry This assumption that there is a magic method for advancing a science is itself a philosophical prejudice dating back to Descartes and Bacon. The effect on psychology of the particular recipe, that scientists must start from the right sort of data, I shall now proceed to to illustrate.

2. Observationalism in Psychology

(a) *The Behaviourist-Introspectionist Controversy.*

One of the most violent disputes about method in psychology was the dispute at the beginning of this century about psychological data. There were heated disagreements between "schools" of psychology who maintained that other people's enquiries were vitiated by the use of data which were scientifically suspect. Lashley, for instance, relegated introspection "to a subordinate place as an example of the pathology of scientific method".[1] Underlying these disputes about data was the observationalist assumption that scientists must *start from* data. It therefore became a major dispute of policy as to which data were to be so favoured. "We are agreed, I suppose", said Titchener, "that scientific method may be summed up in the single word 'observation'; the only way to work in science is to observe those phenomena which form the subject-matter of science."[2] And Watson said, "You will find, then, the behaviourist working like any other scientist. His sole object is to gather facts about behaviour—verify his data—subject them both to logic and mathematics (the tool of every scientist)."[3]
Do not both these statements imply an observationalist view of

[1] Quoted by C. Pratt: *The Logic of Modern Psychology*, p. 49.
[2] E. B. Titchener: *Lectures on the Elementary Psychology of Feeling and Attention*, p. 175.
[3] J. B. Watson: *Behaviourism*, p. 7.

science ? Could not Behaviourists be regarded as deriving their name from their determination to *start from* overt behaviour or visual data instead of from introspective data ? Is not their assumption that the scientist must decide what data he must start from before he can begin his enquiries ? Is he, for instance, to start from " tangible and approachable " rats in mazes or from images and " raw feels " or the " direct experience " of Kohler ?

The Behaviourists accepted what has often been called the Book of Nature view of science just as the introspectionists did. They were opposed to introspection because, in their view, the introspectionists were laboriously perusing blank pages of the Book and mistaking their own finger prints for Nature's secrets. The plight of the introspectionists was due, in the opinion of the Behaviourists, to their having inherited from philosophy a dud subject-matter. " To-day " says Watson, " the Behaviourist can safely throw out a real challenge to the subjective psychologists—Show us that you have a possible method, indeed that you have a legitimate subject-matter ! "[1] Hunter, too, echoes this refrain : " Psychology, unlike the other sciences, has not found it possible to continue with the subject-matter bequeathed it by philosophy."[2]

What, then, was the remedy proposed by the Behaviourists ? Watson can be construed as saying that we must, with the example of the abortive discussions about " imageless thought " in mind, be most meticulous about the data which we feed into the machine of logic and mathematics, like napkins in a washing-machine. Otherwise the machine may grind importantly and " imageless thoughts ", which no one can spot properly, may emerge at the other end like soap-bubbles. The same disastrous results could not possibly be obtained if palpable data like rats and terrified babies were fed in at the start. Similarly Hunter's long-winded homilies on the superiority of his new " anthroponomy " to the old psychology is simply riddled with pointless discussions about subject-matter. The Book of Nature view comes out clearly in such passages as these : " Anthroponomy thus takes its place among the sciences which study specific objects in the environment. Here also belong such disciplines as botany which studies plants, geology which specializes upon the inorganic structure of the earth, and physiology, where the functional activities of the various structures of the body become the subject-matter for

[1] J. B. Watson : *Behaviourism*, p. 17.
[2] W. S. Hunter : " The Psychological Study of Behaviour ", *Psych Rev.*, 1932, p. 7.

investigation." [1] His objection to the Introspectionists is that in studying "consciousness" they were just cataloguing the environment of man—"such things as roses, books, configurations and melodies almost exactly as did the philosophers Berkeley and Hume." [2] But though he is shrewd enough to remark "I think that one great reason for the continued, although apparently waning, popularity of psychology lies in the belief that psychologists are conducting Cook's tours of experience." [3] he never questions the basic presupposition which he shared with the Introspectionists that scientists start from "data" or a specific "subject-matter". His concern is rather to define an alternative subject-matter for anthroponomy—man rather than man's environment. Thus there was no radical criticism of the sort of dicta in which Titchener specialized like "All science is talk, but not all talk is science. . . . Science is orderly and methodical talk, talk that gives a complete and exhaustive account of the subject, talk in which no details are left out which can help us to explain the things talked about. . . . The science of mind must give a complete account and an orderly, well-arranged account of its subject, keeping the facts steadily in view and never running off into mere speculation." [4] Professor Mace, in a yet unpublished article,[5] shows in detail how Titchener mistook philosophical analysis for science. Did not the Behaviourists, in their turn, mistake methodological sermons for science?

In view of the similarity of their methodological assumptions it is not surprising that the systems of the early Behaviourists, when "behaviour" had been substituted for "consciousness", bore a marked similarity to that of the Introspectionists.[6] Wundt started from consciousness as his subject-matter, Watson from behaviour. By the method of experimental observation and analysis Wundt arrived at sensations and Watson at reflexes. These were bound together by principles of synthesis, in Wundt's case the laws of association, in Watson's case the laws of conditioning. In both cases we find the search for simples deriving from Descartes and Locke and a more detailed rendering of Hume's "gentle force".

[1] W. S. Hunter: *Psychology and Anthroponomy.* C. Murchison: *Psychologies of* 1925, p. 97.
[2] W. S. Hunter: *Anthroponomy and Psychology* in C. Murchison: *Psychologies of* 1930, p. 282.
[3] W. S. Hunter: *Psychology and Anthroponomy.* Op. cit. p. 87.
[4] E. B. Titchener: "*A Primer of Psychology,*" p. 2.
[5] *Introspection and Analysis.*
[6] For this point I am indebted to C. Pratt: *The Logic of Modern Psychology,* pp. 6-12.

The Behaviouristic recipe, as one might expect, produced no startling results. Objective methods had been practised for a long time by working psychologists before the Behaviourists presented them as a key to Nature's secrets. In the psychophysical methods, the experiments done on memory and learning, and the conditioned response experiments of the Russians these methods had been used with success. As early as 1905 McDougall had defined psychology as "the positive science of the conduct of living creatures" and rejected the usual definition of "the science of mind" because he insisted on objective methods as well as introspection in the study of behaviour.[1] Even when we turn to a later Behaviourist like Tolman we find, in the main, a translation of existing assumptions into a rather cumbersome behaviouristic jargon. It is true that Tolman, in his *Purposive Behaviour in Animals and Men*, performed a valuable tidying up operation on McDougall by distinguishing between those "drives" which have obvious initiating physiological conditions and terminating states of physiological quiescence and those which have not, by dealing in detail with the cognitive demands of the organism, by eliminating McDougall's introspectable quale as the criterion for distinguishing different instincts, and so on. Also, in making explicit the methodological device of the "intervening variable" (drive, habit, demand, etc.) he turned out a lot of metaphysical lumber from the "hormic" psychology. But it is difficult to find in Tolman a hypothesis which cannot also be found in the work of McDougall, Spearman, the Gestalt psychologists, and previous theories of learning. What is new is his appallingly cumbersome terminology which is dictated by his methodological recipe. Even Freudian assumptions appear in a chapter on "Personality Mechanisms" decked out in behaviouist trappings. The reader's job is mainly one of translation and he is often left to reflect that all this could be said without recourse to such pretentious jargon. In Tolman we find a philosophical *re*-description of what others assumed; we do not find many new explanations. Research seems almost a mere excuse for linguistic ingenuity.

Let it not be thought, however, that the Introspectionists and Behaviourists did not try to practise what they preached. There are cases where scientists indulge in a lot of methodological talk which is contradicted by their practice, *e.g.* anthropologists who claim to collect data about primitive tribes without making any assumptions about them. Certainly Titchener and the introspectionists cannot be accused of this kind of inconsistency. They

[1] In his *Physiological Psychology*.

really took their methodology seriously and tried to study experiences as "existences" without "meanings" which were the concern of the logician. They literally tried to isolate pure "sense-data"—a heroic undertaking. Their attempt to compile an inventory of the mind was a result of taking Locke's injunction to pursue "the plain historical method" seriously. The sterility of traditional introspection derived not so much from the fact that they introspected (or gave retrospective reports on their own doings and feelings), but from the fact that they did this in order to compile an inventory rather than to test fruitful hypotheses. Observation was really regarded as "the Alpha and the Omega of science".[1]

Similarly the Behaviourists avidly turned their gaze upon rats and have more or less feasted their eyes upon them ever since. This concentration on animals renders most of what they say about human beings programmatic. They are not, however, at all modest in issuing promissory notes about the future. Hull, in his recent *Principles of Behaviour* is quite in keeping with the optimism of behaviourists generally when he forecasts a "true behaviouristic science" concerning individual and social behaviour, skills and their acquisition, problem-solving and reasoning, social and ritual symbolism, economic and familial behaviour, individual adaptive efficiency (intelligence), formal educative processes, psychogenic disorders, social control and delinquency, character and personality, culture and acculturation, magic and religious practices, custom, law, and jurisprudence.[2] And all this without committing the grievous scientific sin of using a subject's reports on his own doings and feelings as evidence! Such a science may emerge. The proof of any scientific pudding lies in the eating, not in the recipe for making the pudding. And that is precisely my criticism of the Behaviourists—that they produced a recipe rather than a new theory. Historically speaking Behaviourism was the nemesis of Introspectionism. "All limited methodologies have their day. . . . There is a final epoch of endless wrangling over minor questions."[3] Psychology became, with the Introspectionists, more or less identified with a technique of observation and the minor question was hotly debated as to which kind of observation was to be endowed with a halo. Hull's claim is that Behaviourism alone can save us from the belles-lettres of the "so-called social sciences". Behaviourists did not produce a plethora of fertile

[1] C. Pratt: *The Logic of Modern Psychology*, p. 32.
[2] See C. Hull: Op. cit. pp. 399-401.
[3] A. N. Whitehead: *The Function of Reason*, p. 14.

hypotheses. They did not claim that their theories were better tested than other theories in the field or that more facts could be deduced from them. As a matter of fact their concentration on rats, dogs, cats, and apes prevented them from producing many hypotheses in which other psychologists and social scientists were much interested. Their main principle of explanation was as old as Hume. Their claim was rather than it was only by pursuing a certain method, by starting from the " behavioural data " that scientifically fruitful theories could be obtained. Mind became, as Pratt picturesquely puts it,[1] a " verbal ghost " but the ghosts of Descartes and Bacon materialised in the laryngeal motions of Watson and his followers.

Now, though I have maintained that, historically speaking, Behaviourism was presented as a rationalist recipe for arriving at scientifically respectable hypotheses by starting from impeccable " behavioural data ", I do not want to deny either that there were other reasons for the emergence of this " school " of psychology or that their attack on introspection raises some interesting methodological issues which can be stated in a way which makes them independent of observationalism. If we reject not only observationalism but also the Cartesian view that there is an inner realm of the mental to be observed by special techniques, the question raised by the Behaviourists becomes the practical question of the reliance to be placed on a subject's own retrospective reports of his doings and feelings in testing hypotheses about human behaviour.[2] Though we might maintain that, in principle, there is little difference between my own reports on my feelings and doings and other people's reports on them, in practice, surely, I occupy rather a privileged position. Doctors would find their work rather laborious if they dispensed with symptoms as evidence and relied only on signs. Patients often make mistakes about themselves and sometimes have special motives for cooking the evidence ; but we are not always inaccurate about our own doings and feelings any more than we are always in the position of the hypochondriac or malingerer. In trying to test, for example, a Freudian hypothesis

[1] C. Pratt : *The Logic of Modern Psychology*, p. op. cit. p. 26.

[2] Some might say that the use of retrospective reports is quite consistent with behaviourism as we are then dealing with " verbal behaviour ". (See, *e.g.* Professor Mace, " Some implications of Analytical Behaviourism ", *P.A.S.*, 1948). But this is to use the word " behaviour " so widely that it loses its sting and ceases to do any work. The point is surely that no other organisms use language to *describe* their own doings and feelings and the use of the word " behaviour " in an omnibus way makes us tend to ignore some of the important differences between rats and men.

about forgetting it would seem, prima facie, to be an unnecessary self-denying ordinance to refuse to accept as evidence the report of a subject to the effect that he had a name " on the tip of his tongue " because he was reporting on his own tongue rather than on someone else's. Behaviourists have, in the main, confined their experiments to organisms which are incapable of giving retrospective reports on their own doings and feelings which might be used as evidence in testing hypotheses about them. So the use of introspective evidence has not presented itself as a practical problem in research. Whether Behaviourism, which I interpret as a self-denying ordinance imposed upon the testing of psychological hypotheses, is a wise or practicable prescription in testing more complicated hypotheses about human behaviour involving speech, sympathy, and social conventions, is a question which falls outside the scope of this article. I have been concerned to suggest only that, historically speaking, the practice and methodological discussions of both introspectionists and behaviourists were much influenced by observationalism. I hope that my reference to explicit methodological dicta, to explicit pre-occupation with subject matter, to similarity in principles of explanation, and to the influence of methodology on actual practice has been sufficient to substantiate this suggestion. Others have sufficiently dealt with different aspects of this long-standing controversy [1]; I only claim to have drawn attention to one aspect of it which, because it involves a widespread assumption about scientific method, has tended to have been overlooked.

(*b*) *Operationism in Psychology.*

One of the more recent offshoots of observationalism in psychology is the operationist movement. It is difficult to be clear quite what operationism is because it is not always very clearly stated. Certainly all operationists are rationalistic in the sense that they maintain that unless scientists define their terms operationally they will not uncover Nature's secrets. But what does it mean to define a term " operationally " ? It could mean that hypotheses must be expressed in terms which render them testable by observation. This would be the view about the language

[1] See, for instance, J. Dewey: " Conduct and Experience " in C. Murchison's *Psychologies of* 1930; R. Woodworth: *Contemporary Schools of Psychology* (Rev. Ed. 1949), the famous " Battle of Behaviourism " between Watson and McDougall and the more familiar philosophical commentators like Broad, Russell, Pratt, and Mace.

of science which is parallel to the salutary part of the observationalist doctrine—that scientific hypotheses must be tested by observation. But, if my interpretation of operationist literature is correct, operationism is misleading in exactly the same way as observationalism is misleading. And this is because operationism is an offshoot of observationalism. Just as it is salutary to say that observation is decisive in testing hypotheses but misleading to say that scientists " start from " observations, so also is it salutary to say that scientific terms are meaningful because there are concrete operations by means of which it can be determined whether or not a term is applicable or whether or not what is asserted in a sentence containing scientific terms is true or false, but misleading to say that scientists define terms by means of " operations " or that terms " stand for " operations. There is thus a wider and a narrower interpretation of " operationism ". The wider doctrine is accepted by all scientists who make careful attempts to test hypotheses ; the narrower doctrine is accepted only by those who assume an observationalist view of scientific method. I shall thus be concerned in this article only with the narrower sense of operationism.

This movement in psychology was a reflection of the movement in physics epitomised in Bridgman's book *The Logic of Modern Physics*. There were a number of articles in the *Psychological Review* terminating in a symposium on " Operationism in Psychology " in 1945 as well as a book called *The Logic of Modern Psychology* written by Professor Pratt from an avowedly observationalist stand-point. The operationists tried to apply Bridman's remark that " The proper definition of a concept is not in terms of properties but in terms of actual operations " [1] to psychology. I will quote two typical expressions of the view by psychologists : " Operationism may be defined as the practice of talking about (1) one's observations (2) the manipulational and calculational procedures involved in making them (3) the logical and mathematical steps which intervene between earlier and later statements, and (4) nothing else." [2] This expression has at least the virtue of being clear. The same, however, cannot be said for my other example : " Operational definition makes explicit recognition of the fact that a concept, or a proposition, has empirical meaning only if it stands for definite concrete operations capable of execution by normal human beings." and " A term or proposition has meaning (denotes something)

[1] P. Bridgman : *The Logic of Modern Physics*, p. 6.
[2] B. F. Skinner : " The Operational Analysis of Psychological Terms ", *Psychological Review*, 1945, p. 270.

if, and only if, the criteria of its applicability or truth consists of concrete operations which can be performed."[1] In this latter example we note a shift or wobble. In Stevens' first formulation a concept or proposition has meaning only if it " stands for " definite concrete operations ; in the second formulation it has meaning only if " the criteria of its applicability or truth " consist in concrete operations. This is like saying that the word " horse " stands for my looking, touching, and smelling operation when confronted with (a horse ?) and then going on to say that the word " horse " can only have application if I can look at, touch, or smell a horse. In the first formulation it is clear that the word " horse " stands only for my discriminatory operations (which is a bizarre kind of phenomenalism) ; in the second formulation the more non-committal rule is laid down that I may only use the word " horse " if there are concrete operations by means of which I can decide whether the word is applicable. No mention is made in the second formulation of " standing for " and the question of definition in terms of observed properties is left open. It could thus be interpreted as a statement only of the wider sense of " operationism ".

Operationists claimed to give an account of what scientists in fact do, of how they in fact define their terms. Concepts, they said, find their meaning in specific laboratory experiments ; their meaning must be reconstructed from the context of the scientific activities in which they in fact occur. Stevens makes this explicit when he identifies the much talked of " operations " with " discriminatory responses."

It may well be the case that this sort of analysis is plausible with an example like " length ", where obviously the question " What is the *real* length of x ? " needs the answer " Measuring is something we do to things ". But this hard-headed kind of reply does not necessitate the conclusion that a simple property like extension is defined only in terms of or " stands for " discriminatory operations. The difficulty we encounter with the operationists, especially in psychology, is that they make no effort to sort out the different sorts of terms used by scientists which belong to different levels of abstraction. The only member of the 1945 symposium who does this is Herbert Feigl and he is not an operationist in the narrow sense at all. He distinguishes " qualitative ", " semi-quantitative ", " fully-quantitative ", "causal-genetic" and "theoretical" terms. The extreme operationists, however, make no similar attempt to distinguish these

[1] S. Stevens : " The Operational Definition of Psychological Concepts ", *Psychological Review*, 1935, pp. 517-518.

different levels, and as they talk airily about "properties" as well as about high-level constructs like "force" we must presume that their doctrine is meant to apply to every sort of scientific term—even those that are "ostensively defined".

Surely, too, if the operationists were serious in maintaining that they were giving an empirical account of how scientific terms have meaning, they would produce case-histories of people like Newton, Harvey, and Galileo. In fact this sort of confirmation is not produced. In view of this it is plausible to suggest that their view, far from being an empirical description, was a way of talking necessitated by phenomenalism and observationalism. Do not the operationist extremists assume that scientists *start* with specific operations like "discriminatory responses", measuring operations, manipulations of material, and so on? General terms in science emerge from the correlation of similar operations, and laws and theories from the correlation of measurings. Would not this implicit assumption account for Skinner's insistence that terms stand for nothing more than observations, the manipulational and calculational procedures involved in making them, and the logical and mathematical steps which intervene between earlier and later statements? Scientific laws and theories are presumed to emerge from logical and mathematical operations on the "data" which are observations and measurements.

L. J. Russell, in his review of Bridgman's book,[1] showed that the general theory of operationism is unable, in its narrower sense, to give an account either of error or generality. If "heat" stands for particular operations involved in measuring (heat?) there is no sense in talking of better ways of measuring (heat) or of being mistaken in such measurements. For "heat" stands only for the particular operations involved in measuring and nothing else. Similarly a synapse discovered by different techniques would be a different thing and the meaning of "synapse" would necessarily change not only with changes in the examining devices but also on every occasion on which it was examined afresh by anyone. There is no way of accounting for the generality of terms if it is assumed that we start with "sheer particulars" or that terms are tied down to specific operations. Observation and measurement, as well as other manipulational and calculational operations, presuppose not only things with observable properties to deal with, but also a wealth of previous theory and general assumptions about them. The assumptions which we already have determine the questions we ask, the

[1] MIND, 1928.

observations which we make, and the operations which we perform. We never "start from" such operations. We can and do use different operations to measure length, for instance, because of our existing qualitative knowledge about extended bodies, and it is because of our existing knowledge that we know that it does not matter which end of a cricket pitch we start from if we want to measure it and that we can perform the operation indifferently with a piece of stick, with a piece of tape, or with an iron bar. Our existing assumptions assure us of the equivalence of these operations. But on a strict operationist view the length of a cricket pitch is defined only in terms of measuring operations and the equivalence of the results is inexplicable, as we never measure the same thing, the properties being defined by means of operations. In actual fact we trust a given measurement more if we have another way of making it that acts as a confirmation. This shows the amount of existing knowledge which is presupposed by measurement. We trust measurement with a tape-measure if it is confirmed by triangulation method just as we are satisfied that we have seen a dog in the twilight if we also hear it bark.

There are many other objections to operationism in general which other methodologists, more competent than I, have put forward; these all hold good *a fortiori* of operationism in psychology. There is, however, another point that needs to be made with reference to the application of this doctrine to psychological concepts. My acquaintance with operationist literature is not very extensive, but I have been surprised at the dearth of examples which operationists give to illustrate their thesis. We would expect a list of typical psychological terms ranging from "super-ego", "infantile sexuality" and "Oedipus complex" to terms like "instinct", "drive", "habit", "field", "conditioned response", and so on. Instead, we get not only very few examples but constant citation of the term "experience" and the term "intelligence". "Intelligence is what the intelligence tests test"[1] is a good slogan. But surely care should be taken in erecting a general theory of psychological terms on such a shaky foundation.

It may well be that the slogan "Intelligence is what the intelligence tests test" seems plausible because many testers who constructed battery tests and so on were not quite sure what they were testing, rather than because this approach to the problem of scientific definition was an adequate one. Would it be equally plausible to suggest that kleptomania is (what) the

[1] C. Pratt: *The Logic of Modern Psychology*, p. 79.

psychiatrists diagnose and the courts pronounce judgment on and nothing more ?

The other usual illustration is that of the term " experience " which is another notorious skeleton in the psychologist's cupboard.[1] Stevens points out that experience is, for the purpose of science, to " react discriminatively ". Similarly Professor Mace, who is not an operationist in the narrower sense of the term, is wont to define words like " consciousness " and " experience " in terms of " differential reactions ".[2] In cases like these the operationist thesis is convincing for the simple reason that scientifically useful definitions are being sought for words that may be construed as describing various types of human operations. Talk about consciousness and experience, which has long metaphysical associations, is replaced by talk about discriminatory and differential reactions. As what we are trying to define are regarded as being themselves reactions or operations of organisms it is not surprising that operational definitions (in a literal sense) can be given. To say that A " experiences " x is to say that A " reacts discriminatively " to x. But if the narrower sense of operationism is adopted, to say that A " experiences " x is to say that B " makes a discriminatory response to " (A making a discriminatory response to x) where B is a scientific observer. Biologists, using terms about rats, would always look for the meaning of the terms they used in describing the behaviour of the rats in their own reactions to the behaviour of the rats. This, though flattering to the rats, would be a very bizarre procedure.

Perhaps I can clarify my interpretation of operationists by quoting against them another avowedly empirical account of " meaning ". Charles Morris, in his *Signs, Language, and Behaviour* claims also to be giving an empirical account of sign behaviour. He distinguishes between the interpreter, the interpretant, the denotatum and the significatum of a sign. " Any organism for which something is a sign will be called an interpreter. The disposition in an organism to respond, because of the sign, by response sequences of some behaviour-family will be called an interpretant. Anything which would permit the completion of the response sequences to which the interpreter is disposed because of a sign will be called the denotatum of

[1] See S. Stevens: " The Operational Definition of Psychological Concepts ", *Psych. Rev.*, 1935, p. 520-523 and C. Pratt: " Operationism in Psychology " *Psych. Rev.*, 1945, pp. 262-264.

[2] C. A. Mace: " Some Implications of Analytical Behaviourism ", *P.A.S.*, 1948, Presidential Address, p. 8.

a sign. A sign will be said to denote a denotatum. Those conditions which are such that whatever fulfils them is a denotatum will be called a significatum of the sign. A sign will be said to signify a significatum; the phrase 'to have signification' may be taken as synonomous with 'to signify'"[1] The phrase "stands for" which is so common in operationist literature, seems to be *in ordinary usage* the equivalent of Morris' "signify" and/or "denote". Morris claims that "A merit of the present usage of 'signify' is that it does not make the significatum of every term such as 'spiral nebulae' or 'atom' include biological events, though it recognises that there are no signs which signify without dispositions to respond (that is without interpretants). And since with this usage a sign does not denote its significatum, the temptation is avoided to make the significatum into a special kind of thing—a temptation which seems to underlie the Platonic doctrine of ideas and various philosophical doctrines of 'subsistence'."[2]

Would it not be possible to interpret the operationist view as an over "tough-minded" resolution of conundrums like "What is the *real* length of x?" In their determination to avoid "essences" and metaphysics generally they seemed to by-pass Morris' "denotation" and "signification" and to anchor their signs to earth by equating the "meaning" of a sign or what a sign "stands for" with actual responses or with dispositions to make responses of certain behaviour-families—*i.e.* with the "interpretants" of a sign. Thus, though they dispensed with Platonic heavens, their remedy was so drastic that they must be for ever running round the earth in circles—always defining words in terms of their own responses. But, as Morris points out: "The interpretant, as a disposition to respond caused by a sign, answers to the behavioural side of the behaviour-environment complex; the significatum, as the set of terminal conditions under which the response-sequences to which the organism is disposed can be completed, connects with the environment side of the complex. Each, therefore, involves the other."[3] That a word should have interpretants, as James so often stressed, is a necessary condition of its having "meaning"; but it is not a sufficient condition. We cannot escape reference to properties by speaking only of the observations, and manipulational operations of investigators.

I hope that I have by now succeeded in showing that operationism will not do as an empirical description of how scientific

[1] C. Morris: "*Signs, Language, and Behaviour*", *p.* 17.
[2] C. Morris, *op. cit.* p. 19.
[3] C. Morris, *op. cit.*, p. 18.

terms have meaning, that in general it fails to distinguish between the ways scientific terms at different levels have meaning, that in psychology it derived a certain amount of plausibility from the selection of a few words which are not fair samples of the terminology of working psychologists, and that, though a healthy reaction against metaphysical conundrums, the doctrine in its narrow form is full of absurdities. My suggestion is that the extreme form of the doctrine could only have won the small measure of acceptance which it did win because of the observationalist assumptions of its proponents and of those whom they managed to seduce by their slogans. Operationism, in its narrow form, is a cross between a recipe for scientists and a threat to metaphysicians. Metaphysicians may have trembled but few working scientists have ever attempted to put into practice such a fantastic recipe.

Conclusion

In maintaining that observationalist assumptions formed the basis of the behaviourist-introspectionist controversy and of operationism in psychology, I have, no doubt, over-simplified, many of the issues in order to grind a particular axe about psychology. I do think, however, that my suggestion does provide one explanation of the dearth of fertile hypotheses in a great deal of psychological work. Of course, I emphatically do not want to suggest either that all psychologists have been influenced by observationalism or that this influence has been altogether bad. It is quite obvious, for instance, that Freud was little influenced by observationalism. His interests were technological rather than methodological and technological pressure meant that he *had* to think up hypotheses to explain and cure his patients. Many, however, might maintain that a mild dose of operationism would have done him good in that he might have been led to express some of his hypotheses in a more precise way so that we could know what could count against them. Others, however, might counter that a methodological super-ego would have tended to cramp his style and would have dried up his fertility. They would say that in the early stages of a science we must have bulldozers to clear a passage-way through a jungle as well as underlabourers to convert the passage way into a public thorough-fare. Operationists stop people from talking nonsense ; but if there were too many of them in the vanguard of scientific advance, the pioneers might never talk at all. A balance must somewhere be struck between metaphysics and scholasticism. Today in

psychology there are many who see the key to progress in impeccable data, in operational definition, in postulational techniques and the construction of mathematico-deductive systems; yet all this is of no avail without a lot of bright ideas. Too much pre-occupation with method often distracts people from the necessity of fertile hypotheses. Of course I would not deny that careful and patient experimentation often produces results. Maybe the brilliant hypothesis is often produced by the man who is patient enough to try a thousand others first. My point is that "collection of data" in itself will not lead to progress in a science. The collectors must at least have some questions which they wish to answer and must suggest alternative answers which they can then compare with observation. But there are simply no rules for producing such answers or hypotheses. Some men may get them under drugs, others lying in a bath, others while methodically working in a laboratory using postulational techniques. There must, of course, be some kind of familiarity with a tradition and with what is already assumed in order for scientifically interesting questions to arise. Questions never arise *in vacuo*. But given a question, there is no recipe for answering it in a theoretically powerful manner. I have attacked observationalism not because I do not regard it as vitally important that hypotheses should be tested (with the corollary that they must be expressed precisely enough to permit this) but because I see in observationalism a kind of rationalist recipe which tended to stifle the imagination and to prevent interesting questions being asked. "Some of the major disasters of mankind have been produced by the narrowness of men with a good methodology. To set limits to speculation is treason to the future."[1]

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[1] A. N. Whitehead: *The Function of Reason*, pp. 8 and 30.

V.—NOTES ON THE SYMBOLIC PROCESS

By Ellis Evans

I. The Generation of Relations

Let there be two events, and let there be an event of symbolism between them, so that the first event is symbolised by the second event. Then there are three events; the first event, the second event, and the event of symbolism between them.

However, the third event has now been symbolised by the words 'an event of symbolism between them.' This symbol is a fourth event. There is an *event of symbolism* between the fourth event and the event of which it is a symbol. This event of symbolism is a fifth event, symbolised by the words in italics above. This symbol in italics is a sixth event, and between this symbol and the event it symbolises is an event of symbolism, which is a seventh event, symbolised by the twentieth, twenty-first and twenty-second words in this sentence, which three-word symbol is an eighth event, generating a ninth in the event of symbolism between it and the event it symbolises, and so forth.

It is clear that the preceding account of the symbolic process is not complete, for the ninth event was symbolised in it, giving a tenth event; and at whatever point the account ceases, there will be a further event contained in that account, but not accounted for by it. This last event will be a symbol not recognised as such; that is to say, the symbolic relation between it and what it signifies will not have been mentioned.

The symbol of any event p, we define as $s(p)$. Then the symbol of that symbol will be $s[s(p)]$, which we write as $s^2(p)$. We thus obtain a series of symbols of symbols . . . etc., $s(p)$, $s^2(p)$, $s^3(p)$ . . . $s^n(p)$.

The symbolic relation between a symbol and its referent is defined as $r(p)$, where p is the referent symbolised. Thus $r(p)$ is the relation of symbolism between p and $s(p)$, $r[s(p)]$ is the relation of symbolism between $s(p)$ and $s^2(p)$, and so forth.

The symbol of the relation $r(p)$ will be $s[r(p)]$, the symbol of that expression $s^2[r(p)]$, etc.

Then the relation of symbolism between the relation $r(p)$ and its symbol $s[r(p)]$ will be $r[r(p)]$, which we write as $r^2(p)$, and the further relation of symbolism between $r^2(p)$ and its symbol $s[r^2(p)]$ will be $r\{r[r(p)]\}$, written $r^3(p)$, and so on.

These expressions are non-commutative; $r[s(p)]$ is not equivalent to $s[r(p)]$; the former is the relation between $s(p)$ and its symbol $s^2(p)$, whereas the latter is the symbol of the relation between p and its symbol $s(p)$. S^2, r^3 and so on are convenient shorthand for *ss*, *rrr*, etc.

We now remove the brackets and eliminate the common term p from all expressions. Our generator (the first event symbolised) then is unity, s is the symbol of it, s^2 the symbol of s (etc.), r is the relation arising between 1 (unity) and its symbol s, r^2 the relation between r and sr, and so on. We then have the following table of terms.

1—the primary event symbolised.
s—the symbol of 1.
r—the relation between 1 and *s*.
sr—the symbol of *r*.
ss (s^2)—the symbol of *s*.
rs—the relation between *s* and s^2.
rr (r^2)—the relation between *r* and *sr*.
ssr (s^2r)—the symbol of *sr*.
rsr—the relation between *sr* and its symbol.
sss (s^3)—the symbol of s^2.
rss (rs^2)—the relation between s^2 and its symbol.
srs—the symbol of *rs*.
rrs (r^2s)—the relation between *rs* and its symbol.
srr (sr^2)—the symbol of r^2.
rrr (r^3)—the relation between r^2 and its symbol.
Etc.

Except 1, all expressions are either symbols or relations of symbolism. The first term in each expression indicates whether it is a relation or a symbol.

If an expression is a symbol, the expression formed by its terms minus the first is the referent of that symbol.

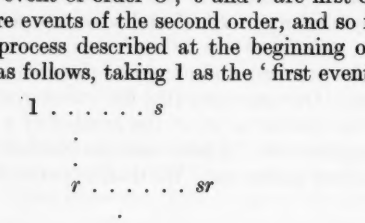
If an expression is a relation, the expression formed by its terms minus the first is the referent which is connected to its symbol by that relation.

Thus *srsr*, being a symbol, is the symbol of *rsr*. *Rsrs*, being a relation, is the relation between *srs* and the symbol of *srs*.

The number of *s* and *r* terms in an expression is the number of symbolic relations, or symbolic operations, involved in the process culminating in that expression. Thus *ssr* (s^2r) indicates three such relations; the relation between itself and its referent *sr*, the relation between *sr* and its referent *r*, and *r* itself, which is the relation between *s* and 1.

We may thus speak of orders of events according to the number of symbolic operations which lead to them. The primary event 1 is an event of order O; s and r are first-order events; sr, ss, rs, rr are events of the second order, and so forth.

The process described at the beginning of this essay can be shown as follows, taking 1 as the 'first event'.



The words 'a tenth event' correspond to sr^4; the 'further event contained in that account', which is not shown in that account, corresponds to the relation between r^4 and sr^4, namely r^5. The words 'a tenth event' are therefore symbols of the fifth order; the unrevealed relation r^5 is a relation of the fifth order; and the words 'a relation of the fifth order' just written are symbols of the sixth order, corresponding to the expression sr^5.

A statement which conveys the three notions of an event, a symbol for that event, and the relation of symbolism between the event and its symbol can clearly be constructed from a symbol conveying the event, a symbol conveying the symbol for that event, and a symbol conveying the relation of symbolism between the event and its symbol. S has the relation r to 1; but a statement to that effect can be constructed by means of a symbol for s, a symbol for r and a symbol of 1. Thus $(s^2\ sr\ s)$ conveys $(s\ r\ 1)$.

Equivalently, a statement to the effect that the words 'the table' symbolise a certain object will consist (constructing a form parallel to that of the notation) of a symbol for the words 'the table', a symbol for the relationship of symbolism, and a symbol for the object table. Using numbers to indicate the order of the words used, we have the statement 'the^2 table^2 means^2 the^1 table^1', which conveys that the first-order words 'the table' have a first-order relation of symbolism to the O-order object

table. Now neither a first-order relation, nor a O-order event (unless it be a mark on paper) can be shown ; but we may indicate the referents of the phrase by ' the^1 table^1 means^1 . . .'. We cannot indicate that the object table is to be thought of by saying ' the^1 table^1 means^1 the^0 table^0 ' as this would indicate that the words after which a ^{0} has been placed are to be considered as non-symbols. No such ambiguity arises with ' means ', however, as the number 1 placed after it indicates that it is the relation we are thinking of ; means^0 would indicate the word ' means ' considered as a mere mark.

($s^2\,sr\,s$), then, conveys ($s\,r\,1$). In order to state, on the same principles, *that* it does, we shall require symbols for the contents of both brackets. These are respectively ($s^3\,s^2r\,s^2$) and ($s^2\,sr\,s$). However, we also have to convey the relations of symbolism which exist between the terms in ($s^2\,sr\,s$) and their referents in ($s\,r\,1$). Taking them term by term, s^2 has the relation rs to s ; sr has the relation r^2 to r ; and s has the relation r to 1. However, since we are to convey these relations, it is their symbols that we require ; these are respectively srs, sr^2 and sr.

Our statement, then, will consist of the terms in the three brackets ($s^3\,s^2r\,s^2$), ($srs\,sr^2\,sr$), and ($s^2\,sr\,s$). The first bracket contains the symbols of ($s^2\,sr\,s$) which is being stated to symbolise ($s\,r\,1$) ; the second bracket contains the symbols of the relations which obtain between ($s^2\,sr\,s$) and ($s\,r\,1$) ; and the third bracket contains the symbols of ($s\,r\,1$). Arranging the terms in three groups, each in subject-predicate order, the first terms in each bracket are ($s^3\,srs\,s^2$), the second terms are ($s^2r\,sr^2\,sr$), and the third terms are ($s^2\,sr\,s$). The completed statement is ($s^3\,srs\,s^2$) ($s^2r\,sr^2\,sr$) ($s^2\,sr\,s$).

Taking the referents of each term we obtain ($s^2\,rs\,s$) ($sr\,r^2\,r$) ($s\,r\,1$), which is a term-by-term explanation of the relations between ($s^2\,sr\,s$) and its referent ($s\,r\,1$), which was what was required.

In equivalent words, the statement ($s^3\,srs\,s^2$) ($s^2r\,sr^2\,sr$) ($s^2\,sr\,s$) would be table^3 means^3 table^2, means^3 means^3 means^2, table^2 means^2 table^1. The referents of the statement are table^2 means^2 table^1, means^2 means^2 means^1, table^1 means^1 . . . , which is a word-by-word demonstration of the relations between table^2 means^2 table^1 and its referent table^1 means^1 . . . (this object).

The next step in the process, the description of the relations between ($s^3\,srs\,s^2$) ($s^2r\,sr^2\,sr$) ($s^2\,sr\,s$) and its referent, would require twenty-seven terms, and would take us into the fourth order.

The term taken as a type of events of order O in a symbolic notation is naturally of similar character to the terms which are

put forward as types of symbols, since both are marks on paper. Ambiguity often arises because of this similarity. Some of the statements we have made, for example, while being in accordance with normal usage, are themselves not in such a form as we have been concerned to reduce statements concerning symbolic relations to in the notation. Thus we have said that '*s* has the relation *r* to 1'; now in that statement we are not using *s* as a symbol, although the words 'has the relation to' are obviously used as symbols. Again, 1 is a O-order event, and it is merely accidental that it may be combined with symbols in statements; but for this accident of form, we could no more say that 1 was an event of order O, or that s had the relation *r* to it, then we could say the object house together with the words 'is an event of order O', or say the object book together with the words '*s* has the relation *r* to it'. However, to be perfectly consistent in our use of language would lead to more confusion than otherwise. Strictly we should say instead of '*s* has the relation *r* to 1' 's^2 has *sr* to *s*'; and that, of course, is what the notation does say. Now, in our notation-statements consisting of three elements conveying (*a*) the symbol to be explained, (*b*) the relation of symbolism which it bears to its referent, and (*c*) its referent; (*a*) and (*b*) are always of the same order, while (*c*) is of an order lower by one. But word-statements may not show the sequence of orders in the same way. 's^3 symbolises s^2', for instance, does not treat s^3 as a symbol at all (*i.e.* it does not convey s^2 when looked at), nor does it treat s^2 as a symbol; but the word 'symbolises' is symbolic. S^3 and s^2 are in fact being treated as O-order marks. They must, of course, so be treated when defined; but the necessity vanishes after their definition. The notation ($s^4\ srs^2\ s^3$) is consistent, for it prevents the ambiguity as to whether s^3 is symbolic or not. Further, there can be no question that we are stating that s^4 symbolises s^3, for the relation between those terms is not srs^2 (which is a symbol), nor rs^2, but rs^3, symbolised by srs^3.

This issue naturally arises in any word-statement containing a predication, although the ambiguity is rarely troublesome when low orders of symbolism are being used. But inverted commas are cumbersome in the higher orders. However, in a word-statement, whatever order of symbol appears in front of a predication, the general alternative is present that if the predicating symbols are of the first order, either the symbol concerned is being predicated, in which case it is not a symbol as far as that statement is concerned, or what it symbolises is being predicated. The notation removes the alternative in the case of a predication

of symbolism ; the predication must concern the referent of the symbol which actually appears.

The consequences of the alternative are interesting in the case of circulating symbols. Let there be two marks, *c* and *d*, each of which symbolises the other. We then have an endless chain of references. Suppose we have the sentence '*c* is black'. This must indicate that either *c* is black and not a symbol in that context, or that *d* is black and *c d c d c d* etc. Similarly '*d* is black' will indicate either that *d* is black and not a symbol, or that *c* is black and *d c d c d c* etc. However, assuming that in such a statement the mark is recognisable as symbolic, the series of reference is usually halted by the convention that whatever the mark symbolises is not symbolic. This gives the simple judgement that '*c* is black' indicates either that *c* is black and not a symbol, or that *d* is black and not a symbol, and similarly with '*d* is black'. This convention is useful in preventing a possible fallacy. It might be imagined that if *c* and *d* symbolise each other, and *c* is taken as symbolic in '*c* is black', the series of references conveys the blackness of *c* as well as that of *d*. But this cannot be the case, for the first-order symbols 'is black' symbolise a quality, normally a O-order event ; and to imagine that the predication is carried on a stage further would be to consider this event as a symbol, or the quality of blackness as symbolising a further quality of blackness. '*c* is black' conveys here, simply, *d*, where the quality is evident as possessed by *d* (if the statement is true) ; now, the reference to *d* may recall *c*, but there is no need to assume any blackness about it. Similarly '*c* is symbolic', conveys that *d* is symbolic, and although *c* is symbolic itself, the statement does not say so, unless *c* is taken as non-symbolic to start with.

What has been said about circulating symbols applies for the most part equally well to self-indicating symbols. Without the convention, '*c* is black', where *c* symbolises itself, produces the notion of *c* being black and *c c c c* etc. With the convention, we accept only the simple notion of the blackness of *c*. *C* symbolises itself as black, and as a non-symbol.

II. Lying

Except insofar as if the referent of a symbol is the universe (*i.e.* everything), the symbol will form part of the referent, there is nothing in a referent to dictate the nature of its symbol. That a referent has certain characteristics does not imply that a symbol for it has them also, and similarly, that a referent may

be subject to a certain judgement or evaluation does not imply the same of a symbol for it. Thus if a certain state of affairs is proposed, which is judged as merely possible or conjectural, and not actual or real, the symbols for that state of affairs are not subject to the same judgement merely by virtue of the referent being so. For if that were so it would imply the impossibility of referring to any merely possible state of affairs. Orchids are not grown in polar regions, but we may say that they are, and such a *statement* is called a lie. Clearly, however it is not the symbols which do not exist, but the circumstances to which they point. Thus the predication of a symbol or set of symbols as a lie implies primarily that a judgement is being passed upon a state of affairs (order O), and certainly does not imply that the same judgement is being passed on the symbols themselves.

Now, to say of a symbol that the state of affairs it signifies does not accord with reality is indirectly to say something about the symbols themselves. But the predication of the symbols is hardly more than verbal; they have the quality 'such that what they point to is not a part of reality.' '*S* is a lie' primarily concerns 1, not *s*. Insofar as a quality may be thought to be predicated of *s* in that sentence, that quality cannot be thought to be predicated also of 1; for we cannot say that a consequence of the sentence is that 1 has a quality 'such that what it points to is not a part of reality', for 1 does not point to anything, as *s* does. Conversely, whatever quality is being predicated of 1 such as the quality 'such that it is not an actual state of affairs' cannot be thought to be predicated of *s*. (It is true that 1 actually exists, but 1 is merely a type of O-order events, which may or may not be real, or may or may not be combined in a way which is 'real'.) Therefore the judgement we are chiefly concerned with in '*s* is a lie' affects the referent of *s*, not *s* itself; and whatever quality may be thought to be predicated of *s* by the sentence is not what we are chiefly concerned with, and is not predicated of the referent of *s*. However, the judgement we are chiefly concerned with in '*s* is red' affects *s* only, and 1 not at all. Thus while '*s* is a lie' and '*s* is red' have a similar form, the effect of the predication is quite different in each case.

Now taking *p* as a type of events of order O, and *n* as the quality of not being in accordance with reality, and assuming the notion of predication to be implicit in the association of the terms, (*sp sn*) will be the notation equivalent to '*sp* is a lie'. Taking *b* as the quality of blackness ($s^2p\ sb$) will be equivalent to '*sp* is

black'. The difference of order, obscured by the word-statements becomes obvious.

If '*sp* is a lie' concerns *p*, what does 's^2p is a lie' concern? Taking the possible O-order event of Charles I having died of old age as *p*, and the statement 'Charles died of old-age' as *sp*, and 'the last sentence' as s^2p, what does 'the last sentence is a lie' concern? *P*, clearly, for it is being said that whatever the last sentence points to is not in accordance with the facts; it is not being said that the last sentence itself is not in accordance with the facts. But 'the last sentence is black' concerns *sp*. Thus in association with second-order symbols whose referent is a first-order symbol signifying a state of affairs, 'is lying' behaves in peculiar fashion. We cannot translate 'the last sentence is a lie' by ($s^2p\ s^2n$), for the referents are (*sp sn*), *i.e.* 'Charles I died of old age is a lie' whereas the referent of 'the last sentence is a lie' is simply 'Charles I died of old age' together with the notion, not the symbol, of a lie. Neither may we translate ($s^2p\ sn$), for the referents are (*sp n*), which is the sentence 'Charles I died of old age' not existing.

If we continue the process and symbolise the words 'the last sentence' (s^2p) by some such phrase as 'the previous three words' (s^2p), the predication becomes more or less ridiculous. 'The previous three words are a lie' conveys little sense if the words referred to do not point to a state of affairs outside the paper they are written on. But if any sense is to be wrung out of 'the previous three words are a lie' it must surely, again, be a judgement about *p*. The point is hardly worth making, as we rarely get any such formations when the words referred to do not indicate a state of affairs in the outside world. But we get many such formations, of course, when they do.

'Is a lie' then, is a peculiar predication; when apparently applying to first or second-order symbols it in fact applies to states of affairs of order O; and such predications cannot be treated as most others can be. In particular, the convention mentioned before cannot apply. If we have 's^2 is a lie' we cannot have the conventional meanings, either s^2 is a lie and not a symbol, or *s* is a lie and not a symbol, for both are absurd, s^2 and *s* not being, in fact, what is being talked about.

With circulating symbols the consequence of setting one in front of 'is a lie' is absurdity, more or less. Similarly, if *c* symbolises *c*, '*c* is a lie' is more or less absurd. One could lay down special interpretations, such as making the sentence mean that *c* did not exist, but that is a personal matter.

However, assuming such a special interpretation as a basis for

argument, 'these three words are a lie', where 'these three words' refer to themselves, is capable of the following interpretations.

(*a*) Taking 'these three words' as first-order symbols, the O-order events of which what 'are a lie' means must be predicated, are 'these three words' seen as non-symbols. The interpretation must then be that (these three words)^0 does not exist, or is not in accordance with reality, or merely possible not actual.

(*b*) Taking 'these three words' as second-order symbols. We have noticed that when associated with symbols of the second order, 'lying' still concerns the referent of their referent. This is (these three words)^0, and the interpretation must be the same as before.

(*c*) Taking 'these three words' as symbols of some higher order. Ordinary usages do not give much guidance. However, the predication must still concern (these three words)^0, the only O-order events even remotely concerned.

(*d*) Taking 'these three words' to be true self-indicating symbols and of infinite order. In this case there are no O-order events to which the predication can apply, and the sentence can have no meaning beyond that of 'are lying'. We may, of course, say that 'these three words' have a quality such that the O-order state of affairs they point to does not accord with reality, but this is a mere verbalisation of the essential position.

The 'liar' paradox can now be dealt with. This reduces to the form 'this sentence is lying'.

The words 'this sentence' symbolise both themselves and the other words in the sentence. We may thus divide the sentence into two groups of symbols, $s(s + sn)$ and sn, remembering that s symbolises $(s + sn)$ whenever it occurs.

Taking 'this sentence' as a first-order symbol, the O-order events it points to must be $(s + sn)$ seen as non-symbols. Predicating of them what is meant by 'is lying', we get some such interpretation as '(this sentence is lying)^0 does not accord with reality.'

If 'this sentence' is a second-order symbol, it symbolises 'this sentence is lying', which symbolises '(this sentence is lying)$^0 + n$'. Predicating n of this, we get something like '(this sentence is lying)^0 does not accord with reality' 'does not accord with reality', which seems to amount to an assertion that the meaningless marks are real, and not merely possible.

Taking 'this sentence' as a third-order symbol gives a

predication by n of a predication by n of a predication by n of (this sentence is lying)^0. Similarly, taking 'this sentence' as a symbol of any higher order will result in a series of predications, reducing (if one accepts them as cancelling each other) to a predication of certain meaningless marks as real or unreal according to the number of predications allowed, or rather, the number of symbolic operations admitted.

Taking 'this sentence' as truly self-indicating, and of infinite order, results in no predication of any meaningless marks as anything, for the symbols 'this sentence' recur. There is theoretically an infinite series of predications by n, but no ultimate subject for them.

The above interpretations are deliberately very broad. Normal usage, as we have seen, does not allow much sense to a predication of a symbol higher than the second order by 'is lying', and whether or not one admits the possibility of meaningless marks being what is meant by 'lying' seems more or less a personal matter. However, that is what an insistence on the sentence being taken as a predication of itself results in. 'This sentence is black' is fully significant, because it is immaterial whether the predication concerns symbols or meaningless marks; but the predication in 'lying' cannot concern symbols primarily, as we have seen.

There are three chief factors in the mechanism of this paradox; first, the self-indicating symbol; secondly, the ambiguity of the predication involved in 'lying'; thirdly, the analogy with significant forms with self-indicating symbols on the one hand, and 'lying' on the other. The self-indicating symbol produces an infinite series of references, which must be halted if a predication is to be obtained. The 'lying', which seems to predicate symbols, disguises the fact that, if the series of references is halted, the self-indicating symbol must refer to itself as a mere mark. The analogy with other forms disguises the artificiality of the sentence. The combination is neat, and bewildering, but not really a matter for logic. There is no point in deciding the order of the proposition being asserted until one is sure what is being asserted. One cannot decide that without a more or less arbitrary decision as to whether meaningless marks can be predicated as n, or what is meant by 'lying', and where one is to stop the series of references, and whether a predication of what is meant by 'lying' by itself reinforces or contradicts the value-judgement involved. The interpretation closest to normal usage is perhaps that arising from considering 'this sentence' as a first-order symbol, which gives the notion of meaningless marks

not according with reality. But deciding on an interpretation is not the chief point, which must be the recognition of the source of the paradox in the combination of symbols which refer only to themselves and to other symbols, with a predication which concerns only O-order events, and symbols only insofar as they point to O-order events. In fact, 'this sentence is lying' never predicates anything of itself, unless 'itself' is considered as meaningless for the purpose of the predication.

It may now be objected that while this kind of investigation may show how paradox arises in particular cases such as 'this sentence is lying', it does not attempt the general question of a statement which does, in fact, predicate of itself that it lies. Further, it may be said that while 'this sentence is lying' may be a confused construction, and may mean very little when interpreted along conventional lines, it does, nevertheless, convey the notion of predicating of itself that it lies, whatever its symbols may convey in other contexts. The semanticist who says otherwise then appears as a mere pedant insisting that the real 'meaning' of an English word is the meaning of the Latin from which it is derived.

One could suggest that all statements which seem to say that they lie contain self-indicating symbols and involve confusions similar to those in 'this sentence is lying', and one could probably show this to be true of any such statement produced. But this is really no reply to the hypothetical position, whereby there is a *notion*, as distinct from a statement, which involves its own falseness; the idea of that idea being false. Further, what has been said about 'this sentence is lying' applies almost equally well to 'this sentence is true'. Is there then a notion of the truth of that notion, and a notion of the falseness of that notion; for if there is, the paradox would seem to be raised again on the level of thought, not of language; the paradox would seem to remain that, given the notion of the assertion of the falseness of that assertion, the truth of the assertion must follow. Is it then possible to conceive of the simultaneous truth and falseness of something?

The writer's view is that there is no such conception or notion. This seems to admit a further paradox, for if there is no such notion, how can I express it in order to deny its existence? My reply must be that I have not expressed the notion; I have expressed two notions in sequence—'truth and falseness'—and added the qualifying notion of simultaneity—'the simultaneous truth and falseness of something'. It is possible, for instance, to run and sing at the same time; however, the fact that

one may say ' simultaneous running and singing ' does not imply that the combination is actually possible. The analogy between the two constructions however, permits the illusion of there being a single notion of the truth and falseness of something, and it is that illusion which allows me to deny the existence of the notion by appearing to express it.

Can, then, a form of words be found which does not rely upon such false analogy in order to convey the notion of simultaneous truth and falseness ? That is, can the simultaneous assertions be made without expressing the notions of truth and falseness in sequence and declaring them to be simultaneous ? For if not, we are hardly justified in saying that the notion is possible ; although of course a person may declare that he can conceive the notion, even though he cannot express it except in a sequence. I think it probable that any attempt to find such a form of words would merely produce some variant of ' I am lying '. If that is so, and if, as I think almost certain, such variants would be subject to much the same semantic analysis as ' this sentence is lying '—that is, they would be shown to be semantically confused—then, I think, the liar paradox has been solved finally by the semantic approach. The question really depends on whether one admits that it is possible to have a conception which it is impossible to express, but which is the sort of conception which can find its place in a philosophical discussion whether mental or verbal. For if it is not the latter we have no business with it. If we admit the possibility of such a conception there would not seem to be much point in discussing basic notions. If on the other hand we dismiss the possibility, then we require that the conception be expressed. If it cannot be expressed we must have nothing to do with it. But if it is claimed that it can be and has been expressed, we are entitled to examine the statements which are said to express it. Now, if those statements express the notions of truth and falsity in sequence and declare them to be simultaneous, they do not, as we have seen, express the notion of the simultaneous truth and falseness of something, but merely the notion of something being true, the notion of the same thing being false, and the notion of simultaneity. If, on the other hand, the statements are of the ' I am lying ' type, they are open to semantic criticism. If no other kind of statement can be found of which it is claimed that it expresses, or implies, that something is both true and false at the same time, we can turn aside from the question justifiably.

III. An Illicit Completion

A quality may be made known by either or both of two methods. It may be made known by demonstration, without the provision of a definition; or it may be made known by the provision of a symbol or group of symbols forming a definition.

Let us take the former case; then the quality is known without reference to defining symbols. What are the possibilities of answering the question whether a symbol for the quality possesses it or not?

In order to answer it, reference must be made to the qualities actually possessed by a symbol for the quality. However, these are largely indeterminate, for there is nothing in a referent to dictate the characteristics of a symbol for it, unless the referent is the universe. There is no more necessity that one word should symbolise the quality than that a million should, and no necessity that the symbol should be written or verbal. Self-indicating symbols being not unknown, a perfectly possible symbol for the quality would be quality itself. Thus the only qualities that a symbol for the quality must have, are those which every symbol must have, by virtue of its existence and significance. Let us call such qualities, essential qualities. Then, the other qualities of the symbol being indeterminate, the question resolves into, is the quality we are discussing an essential quality or not? If it is, then its symbol must possess it, regardless of the nature of the symbol; if not, it cannot be said, regardless of the nature of the symbol, whether a symbol for the quality possesses it or not.

Now the quality we are discussing is the heterological quality, and to suggest that it is an essential quality of all symbols is absurd. Therefore it cannot be said whether a symbol for the quality possesses it whatever the symbol's nature, and therefore there is no general solution possible.

We are entitled to say, then, that granted only that the heterological quality is not an essential quality, the consequence of attempting to answer the question, is heterological heterological? without reference to a definition in terms of other symbols is the impossibility of an an answer to the question. But we also notice that no paradox has arisen.

All verbal explanations of symbols can be reduced to the form of an equivalence stated between the symbol to be explained, and another symbol or group of symbols. Then in such a statement, the symbol for the symbol to be explained must be of the second order at least; for if it were the first (s) the thing

about which it is saying something would be of order O. Also, since the symbol to be explained and the symbols in terms of which it is explained are equivalent (equivalent symbols being those with the same referent), they must be of the same order. Then, letting the symbol to be explained be $sx(a)$, and the others $sx(b)$, the statement will be $(s^2x(a) = s^2x(b))$. We take the equal sign to be a first-order symbol of the fact of equivalence.

Whatever the form of words in which an equivalence to 'heterological' is framed, at least two notions or groups of notions must be conveyed by it, namely the notion of not possessing (a quality), and the notion of a quality being symbolised.

The equivalence "'heterological' means the same as 'not having the quality symbolised'" is $(s^2x(a) = s^2x(b))$. But since we have divided the basic notions comprising the quality into two groups, we shall construct a composite symbol from $s^2x(b)$. This will be $(s^2x(c) + s^2x(d))$, and the new equivalence is $(s^2x(a) = s^2x(c) + s^2x(d))$. "'Heterological' means the same as 'not having (+) the quality symbolised.'"

We now predicate the quality, of the symbols for the quality. In order to predicate anything of a symbol, we must have a second-order symbol at least. The second-order symbols are $s^2x(c)$ and $s^2x(d)$. In order to predicate a quality of anything, we need first-order symbols for the quality, usually. These are $sx(c)$ and $sx(d)$. The predication therefore is '$s^2x(c) + s^2x(d)$ have the quality $sx(c) + sx(d)$.' Equivalently, "'not having the quality symbolised' has the quality of not having the quality symbolised," or, "'heterological' is heterological."

Let us consider the referent of $sx(d)$; or of the words 'the quality symbolised.' Does $sx(d)$, and do those words, refer to a quality which is symbolised by something outside the sentence, or do they refer to a quality symbolised inside the sentence?

Evidently, they refer to the quality which is symbolised by the word 'heterological' which quality is also symbolised by the equivalence to 'heterological', namely the words 'not having the quality symbolised.' However, these words are rendered by two sets of symbols in our notation statement; $s^2x(c)$ and $s^2x(d)$; and $sx(c)$ and $sx(d)$. The referents of the first group are symbols; the referents of the second group are the notions comprising the quality. Therefore the quality to which $sx(d)$ refers, and to which 'the quality symbolised' refers, cannot be a quality symbolised by the first group, for they do not symbolise any quality, but merely symbols; thus the quality to which $sx(d)$ refers must be the quality to which the second group refer. Therefore the referent of $sx(d)$ is the referent of $sx(d) + sx(c)$.

However, there is no need an for infinite regress. We have only, when reading $sx(c) + sx(d)$, to begin with $sx(c)$, understand its referent, go on to $sx(d)$, understand that its referent is whatever it is plus the referent of $s(c)$, and then stop. $Sx(d)$ does not refer to itself as 'this sentence' does. We have merely previously agreed to regard its referent as being whatever the referent of both symbols is. We may continue to look at it with some degree of curiosity, but that is up to us; it does not tell us to do so.

Thus the statement $sx(c) + sx(d)$ is wholly equivalent to $sx(c) + sx(c)$.

This conclusion is curious enough to suggest a more detailed examination. Let us consider more closely the notion of a symbolised quality, and in particular the referents of the phrase 'the quality symbolised'.

The words 'the quality' do not, as they stand, convey a particular example of a quality, but the general notion of a quality. The word 'symbolised' conveys the notion of something acting as referent.

Setting the two together in 'the quality symbolised', what is the result? Do they convey

(*a*) the *notion* of a quality acting as referent, or

(*b*) the notion of a *quality* acting as a referent?

If (*a*), then the words 'the quality symbolised' convey the general idea of a quality, and the notion of that general idea being symbolised somewhere. If (*b*), the words convey the notion that somewhere a particular quality is being symbolised.

Following possibility (*b*), can that particular quality be one which is being symbolised in the words 'the quality symbolised' themselves? Not as regards the words 'the quality', for they convey the general notion of a quality. But as regards the word 'symbolised', this *might* be taken as indicating a particular quality, *i.e.* the quality of being symbolised. But in that case the meaning of the phrase would be that of 'the quality, *i.e.* the quality of being symbolised', as one *might* say 'the quality looked at' meaning 'the quality of being looked at', 'the quality "looked-at"'. This interpretation is due to a grammatical accident, therefore we dismiss it. It arose from 'symbolised' being taken as indicating the particular quality we are supposing is being talked about under the terms of possibility (*b*). Thus 'symbolised' cannot indicate that particular quality here, and we have already accounted for the words 'the quality'. There being no more words in the phrase, that disposes of the possibility that, if (*b*), the particular quality is one which is being symbolised by words in the phrase. Therefore, if (*b*), the quality

is being symbolised by words other than those in the phrase 'the quality symbolised'.

Now, if (*a*), the words 'the quality symbolised' indicate the general idea of a quality, and the notion that that general idea is being symbolised somewhere. That 'somewhere' may be either in the words, or outside the words. If in the words, it can only be by the words 'the quality'. We thus have the following three possibilities of interpretation, the first two deriving from (*c*), the third from (*b*).

1. That 'the quality symbolised' means that the general notion of a quality is being symbolised in the words 'the quality'.

2. That 'the quality symbolised' means that the general notion of a quality is being symbolised by a symbol not occurring in the phrase.

3. That 'the quality symbolised' means that a particular quality is being symbolised somewhere by a symbol not occurring in the phrase.

In our definition of the heterological quality, we say that it is the quality of not having the quality symbolised. If 1., the quality is that of not having the general notion of a quality. We do not mean that. If 2., the quality is that of not having the general notion of a quality, which general notion is not conveyed by the words 'the quality'. We do not mean that either. If 3., the quality is that of a symbol's not having the particular quality it symbolises. That seems to be what we mean, but we notice that possibility 1 is the only one which allows the quality referred to by 'the quality symbolised' to be something conveyed within that phrase.

Therefore in the equivalence to 'heterological', *i.e.* 'not having the quality symbolised' the quality referred to by 'the quality symbolised' cannot be any quality thought to be conveyed by any of those three words. But the quality referred to must be that referred to by 'not having the quality symbolised'. Therefore 'not having the quality symbolised' is equivalent to 'not having not having', $sx(c) + sx(c)$, and other interpretations must arise from a misapprehension.

What is this misapprehension? When we say, of a heterological adjective, that it does not have the quality symbolised, we have in mind usually a distinct and familiar quality conveyed by a word which is not at all like anything like that occurs in 'not having the quality symbolised.' Such a word (*e.g.* 'red', 'monosyllabic', 'invisible') is usually a common enough adjective, and the quality it conveys is usually part of our familiar world. But it is possible to construct verbal qualities which are

not familiar in the least ; qualities such that . . . , and easy to construct a single adjective which is defined as equivalent to the such that . . . symbolism. Now the problem demands that we make ' the quality symbolised ' equivalent to ' not having the quality symbolised '. This gives ' not having not having ', but very naturally it is still *felt* that there must be somewhere a quality referred to by ' the quality symbolised ', other than whatever ' not having the quality symbolised ' means. A quality in fact, such as we know is conveyed by ' red ', etc. This quality cannot be shown, for it does not exist ; but it is thought to exist, and since it is thought to exist, it is thought to be capable of expression in the words ' the quality symbolised '. Therefore the words are added to ' not having not having ', we obtain ' not having not having the quality symbolised ', and a paradox.

Two other considerations, I think, help this forward. One is the feeling that ' not having ' is not really symbolic of a quality. However, unless it is taken as a quality, or as part of a quality, the paradox cannot be posed, since we only understand ' heterological ' from a definition such as ' not having . . . etc.' The other point is the grammatical one. Perhaps years of ' completing the following sentences ' at school make the provision of an object for ' not having not having ' automatic ; unfortunately the object provided doesn't mean anything.

This cannot be a complete solution, as it is always possible to insist on a different equivalence to ' heterological ', but I think this example indicates a general line alone which a solution might be provided in each case, and may be complete in that sense. The equivalence might contain a self-indicating symbol (although I don't know that such an equivalence would be a very good one) in which case the solution would be a little different but not materially so, since it seems that some of the symbols in the equivalence would be required to have the same referent as all the symbols, whatever the form of the equivalence.

There remains a possible objection that what we have called an equivalence, may, in the definition of ' heterological ', be taken as the referent of that word. In this case, the defining statement would be some such sentence as " ' heterological ' means ' not having the quality symbolised.' " But it can be shown that this is equivalent to (s^3 *srs* s^2), whose referents are (s^2 *rs* *s*), ' heterological ' being of the third order in the defining statement, symbolising a ' heterological ' of the second order. The question then asked, is heterological heterological ? must ask the question of the ' heterological ' the defining statement talks about, which is a second-order symbol. Thus the question

asks whether a symbol of a symbol has the quality it symbolises, and is absurd, as it does not symbolise any quality. The paradox cannot therefore be stated if 'heterological' is defined as a symbol of certain words.

It might also be said that there are many ways in which signs refer to things, and there is a difference between what a word symbolises and what it indicates. 'My house' and 'the house at the end of the street' symbolise different conceptions, but indicate the same thing. It is quite true that in this account no such distinction is made between what 'the quality symbolised' and 'not having the quality symbolised' symbolise on the one hand, and indicate on the other. However, to insist that the distinction should be made is to deny the paradox at the outset; for 'heterological' must be known by definition if the paradox is to be stated at all, and definitions reduce to equivalences. Such an objection cannot stand against the legitimate 'assume that an equivalence to heterological can be formed'; for that is an assumption of the kind we make whenever we say that a word means so-and-so, and is unavoidable. In any case, the paradox can be stated without the use of the word 'heterological'; one need merely ask whether 'not having the quality symbolised' has the quality it symbolises. It could hardly be maintained that the second-order symbols there referred to something in a materially different way from that in which the first-order symbols did. So a question of the confusion of kinds of reference hardly arises; the source of the paradox is something quite different; the illicit completion of a predication.

That is the work of analogy, just as the assumption that 's^2 is a lie' is the same kind of statement as 's^2 is black' is the work of analogy. I have tried to show that a semantic approach can reveal false analogy at work in language used to discuss rather fundamental issues arising out of the liar paradox. It may well be that these analogical constructions confuse other important issues as well; if so, a semantic approach might clarify them.

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VI.—OSTENSIVE PREDICATES

By S. Körner

Philosophers frequently fail to distinguish between the precise description of imprecise relationships and the replacement of imprecise relationships by more precise ones. A failure to make this distinction seems to strengthen two opposite mistakes in philosophical method. These are the misdescription of inexact relationships as if they were exact and the erroneous belief that imprecise relationships require imprecise descriptions. The purpose of this article, which is mainly an attempt to describe imprecise relationships in a precise manner, is to consider the logic of ostensive predicates for its own sake and to show its relevance to some wider philosophical questions. In Section I I shall by discussing a widely held dogma of philosophical logic draw attention to the general inadequacy of Boolean algebra and related systems for the description and understanding of the way in which ostensive predicates are used. In Section II the possible relations between ostensive classes will be considered. This will provide a justification for the distinction between two kinds of logical necessity. In Section III I shall discuss a typical replacement of a relation between ostensive classes by a relation between non-ostensive classes which has some relevance to the question of applying mathematics to empirical fact. In Section IV the traditional distinction between assigning an ostensive predicate to existing and to conceivable things will be rejected as being incompatible with the function of ostensive predicates.

I

Ostensive predicates are words or other potential labels in so far as their use is guided by ostensive rules. Perhaps the most familiar way of formulating an ostensive rule is to say with appropriate pointing gestures that this and this and everything like it is to be assigned a certain word. An ostensive rule involves the choice of a label, an exemplification, and a "like it" or comparative clause. The exemplification need not be performed by pointing gestures. It is often difficult to convey an ostensive rule to another person. These difficulties, however, can for our purpose be disregarded as incidental. For that purpose everybody might limit himself to the consideration

of ostensive rules formulated by himself for himself. An ostensive rule is understood if its exemplification can be extended by a further example or if an " antiexample " can be given, *i.e.* a case indicated where the label is not to be assigned. The rule does not limit the number of further examples or anti-examples. One may, of course, consider an ostensive rule without accepting it, that is without ever acting in accordance with it or without being inclined to act in this manner. On the other hand one might accept an ostensive rule, like any other rule, without considering it. To every ostensive rule there corresponds what will be called an " ostensive class ". This is given by means of an exemplification and a comparative clause and is not given in any other way. Two ostensive rules may differ in the choice of label only, but this difference can again be disregarded. If two labels do not differ in the way in which they are assigned, it is customary to say that they express or designate the same concept. It seems, however, preferable to say that they are used as the same concept for this way of speaking does not suggest that that to which a label is assigned, is in every case a concept. On any meaning of " concept " such a suggestion would be mistaken.

In order to bring out the peculiarities of ostensive predicates it is useful to discuss a thesis apparently accepted by many philosophers who hold that all necessary propositions are analytic; the thesis that every inclusion has a complement. It can be formulated more precisely as follows : Assume that two labels " P " and " Q " are assigned in such a manner that the class *P* (by which we shall mean the class of things to be labelled " P ") is included in, but does not include, the class *Q*. Then *there exists* a class *C*, the complement of the inclusion. It consists of all things which are to be labelled " Q " and are not to be labelled " P ". The complement of the inclusion of *Father* in *Male* is *Male and not a Father*.

In spite of its apparent clarity the thesis is obscure. This is mainly due to the occurrence of the phrase " there exists " in its formulation. The air of self-evidence surrounding it derives from a trivial interpretation which must be mentioned before it can be disregarded. I mean the interpretation according to which to assert the existence of a complement is merely to assert its being postulated. The following second interpretation of the dogma that every inclusion has a complement is, however, more promising : It is self-contradictory to assert (*a*) that two labels " P " and " Q " are in accordance with two rules assigned to things in such a manner that the class *P* is included in the

class Q and (*b*) that a third label say " C " *cannot* be assigned in such a manner that C is the complement of the inclusion. Even in this interpretation an obscurity is left. What, it might properly be asked, is a rule which assigns labels to things ? A satisfactory answer to this question cannot be given by arbitrary legislation. One might be certain that the rules guiding the use of " and " do not assign it to anything, and less certain about the rules guiding the use of " proposition ". Nevertheless, one can point to one kind of rules which certainly assign labels to things, namely to ostensive rules. For by " rules assigning labels to things " we either mean ostensive rules or we mean ostensive rules together with other rules which are in some important respect similar to ostensive rules.

If one adopts the proposed interpretation of the thesis that every inclusion has a complement, then it can easily be seen to be false for ostensive classes. The use of two predicates " P " and " Q " may be guided by *ostensive* rules in such a manner that although the class of things to be labelled " P " is included in the class of things to be labelled " Q ", it is yet not self-contradictory to assert that no label, say " C ", can be used in accordance with an ostensive rule in such a manner that the class of things to be labelled in accordance with this rule is a complement of the inclusion. This might seem ludicrous to those who regard every class as definite, all class-relations as Boolean and who consequently visualise these relations by means of Euler diagrams. A Boolean class-inclusion, as we see in almost every text-book on logic, can be represented by the class of houses in a town whose border is fixed and does not touch any house and the class of houses in a county whose border is similarly fixed assuming that the county surrounds the town. It is then, of course, always possible to indicate the houses which lie in the county but not in the town.

We might, however, assume that we know that the town lies within the county, that we know of some houses that they lie in the town and therefore in the county, that we know how to extend the exemplification of these houses, but that we nevertheless do not know the borders of either the town or the county. We then cannot exemplify any house as being in the county but not in the town. In asserting this there is consequently no contradiction involved. There is similarly nothing in the nature of ostensive rules which makes the assertion self-contradictory that *some* inclusions between ostensive classes have no exemplifiable and thus no ostensive complements.

If the thesis that every inclusion has a complement is false

for ostensive classes a further assertion must also be false. This is the assertion that every inclusion has what we might call a "supplement". If the ostensive class P is included in, but does not include, the ostensive class Q, then the inclusion has a supplement if and only if, *there exists* a class S which fulfils the following conditions: (1) P is included in S, (2) P consists of all things which belong to both Q and S and of no other things, (3) the inclusions of P in Q and of P in S have complements. The inclusion of *Father* in *Male* has the supplement *Parent*. The dogma that every inclusion has a supplement is false for ostensive classes because the third condition is not necessarily fulfilled for every inclusion between ostensive classes. The reason why the dogma is false lies in the nature of ostensive rules. That it is false has been known for a long time. W. E. Johnson's discussion of the relation of the determinates under a determinable to this determinable and to each other amounts to a denial of this dogma which underlies a great deal of modern epistemology and philosophical logic.

It does not fall within the scope of this article to construct in detail a non-Boolean algebra which would adequately reflect the use of ostensive predicates. A further feature of their logic seems, however, worth mentioning and clear on reflexion. Just as class-inclusions can but need not have ostensive complements and supplements, so the logical sum of two classes can but need not be ostensive. As an illustration one might consider the class of things to be called "green" and the class of things to be called "round". The logical sum of these two classes consists of the things to which "green", the things to which "round" and the things to which both these labels are to be assigned. This sum-class is not ostensive. It does not consist of things to which a potential label can be assigned in accordance with an ostensive rule (to the effect that this and this and everything like it is to be given a certain label). The rule guiding the use of "green and round" although a compound of two ostensive rules is itself not ostensive.

II

Most people who reflect on the matter would distinguish between the following logically necessary assertions as belonging to three different types: (1) 'What is green is necessarily coloured', 'What is green is necessarily not red', 'What is green is necessarily extended', (2) 'Whatever is a crow is necessarily black', (3) 'Whatever is a circle is necessarily a conic

section '. If assertions are persistently put into different classes because they are "felt" to be different, the philosophical task arises to show that the distinction is or is not justified. To justify a distinction is to give clear criteria for a classification which is made without any criteria or by means of criteria which are not clear. In this section I shall argue that the distinction between the three types of necessary proposition can be justified mainly in terms of the different kinds of class-relations between ostensive classes. I shall not discuss other attempts to justify the distinction or attempts to show that the distinction is not justified. (I especially apologise for not examining two interesting recent papers by Mr. Toulmin and Mr. Pears as I could not do justice to them in a few sentences.)

The distinction between necessary assertions of type (1) and (2) on the one hand and those of type (3) on the other is justified by pointing out that only the latter relate non-ostensive predicates. It remains to distinguish clearly between assertions of type (1) and type (2). To do this we consider the possible class-relations between ostensive classes. Two Boolean or definite classes can only stand in the relation of (*a*) bilateral or unilateral class-inclusion, (*b*) class-overlap, (*c*) class-exclusion. For these classes *quartum non datur*, just as in the case of two areas in a plane either one lies within, overlaps with or excludes the other. In the case of ostensive classes two further class relations are possible namely (*d*) class-inclusion-or-overlap, (*e*) class-exclusion-or-overlap. (In a strictly formal treatment one would have to consider the further possibility of exclusion-or-overlap-or-inclusion.) In order to illustrate these additional class-relations we decide to use the words "white", "black" and "crow" *only* in accordance with the ostensive rules guiding their use. These rules entitle us only to say that *black* includes-or-overlaps with *crow* and that *crow* excludes-or-overlaps with *white*. It is, of course, quite possible to add to the ostensive rules for these words further rules which, so to speak, decide between exclusion or overlap and between inclusion or overlap. Such a further *non-ostensive* rule might be the rule to the effect that a crow is by definition black.

Ostensive predicates of the natural languages are used in accordance with ostensive and other rules. It is, however, possible to suppose that one has accepted only the ostensive rules guiding the use of two ostensive predicates and to consider the class-relation which is associated with the acceptance of these ostensive rules only. This class-relation we shall call "the primitive class-relation associated with the two ostensive

predicates ". The primitive class-relation associated with two ostensive predicates may and very often does differ from the actual class-relation which is associated with them and which corresponds to the acceptance of all rules guiding their usage. The primitive class-relation of " crow " and " black " is overlap-or-inclusion, the actual class-relation is inclusion.

The distinction between the five possible class-relations of ostensive classes and the distinction between primitive and actual class-relations associated with ostensive predicates allows us to justify the distinction between logically necessary assertions of type (1) and of type (2). A class-relation associated with two ostensive predicates is of type (1) if, and only if, the actual and the primitive class-relation associated with these predicates are both of the same kind and are both either class-inclusions, class-exclusions or class-overlaps. Thus both the actual and the primitive class-relations associated with " red " and " extended " are class-inclusions, with " red " and " green " class-exclusions, with " red " and " round " class-overlaps. On the other hand the actual and the primitive class-relations associated with " black " and " crow " are of different kinds.

It is usual to regard logically necessary assertions of the first kind as being dependent on reality in a way in which those of the second kind are not so dependent ; and to regard assertions of the second kind as dependent on arbitrary convention in a way in which assertions of the first kind are not so dependent. It appears from the preceding discussion that this account, although dangerously vaguè, is not wholly unjustified. On the one hand primitive class-relations correspond to ostensive rules only. To apply an ostensive rule is to state a likeness which is not dependent on anybody's stating it. Indeed, that something is like some other thing does not entail that any person states anything whatever. On the other hand the non-ostensive rules whose application involves the replacement of the primitive inclusion-or-overlap or exclusion-or-overlap by inclusion, exclusion or overlap are of a different nature. To apply such a rule is in a way not merely to state a relationship but also to create it within the possibilities left open by the primitive class-relation.

III

Human beings at a certain level of their development make statements which are, *prima facie* at least, non-ostensive. Thus they assert that certain things belong to non-ostensive classes. An example of such a class-member assertion is the statement

that what is expressed in the preceding sentence is a proposition. They further assert that non-ostensive classes stand in certain class-relations. An example of such a non-ostensive class-class-assertion is the statement that the class of geometrical circles and that of geometrical ellipses (with two different foci) are exclusive of each other. In this section I shall consider the elimination of border-line cases by means of replacing ostensive class-class-assertions by non-ostensive ones. The possibility of this interchange of ostensive and non-ostensive class-relations is connected with the question of what is involved in applying mathematics to experience.

To the question as to what is involved in " applying " non-ostensive statements of mathematics to ostensive statements describing experiences two kinds of answers have mainly been given. Answers of the first kind amount to the view that mathematics contains class-member assertions about certain non-ostensive particulars ; that these particulars are similar to ostensive particulars ; that in virtue of this similarity the non-ostensive statements of mathematics are approximately true of ostensive particulars. Answers of the second kind amount to saying that mathematics is the science of drawing inferences. To apply mathematics to ostensive assertions is simply to draw deductions from ostensive statements in some well-regulated manner. On this view mathematics contains no class-member assertions, neither ostensive nor non-ostensive ones. This second type of answer does not require non-ostensive particulars which have to be located outside space and time. It involves, however, difficulties of its own. Thus, it is clear, that in many cases where one speaks of applying mathematics one does not directly draw conclusions from ostensive assertions. One first, replaces ostensive by non-ostensive assertions. Only after this replacement has been accomplished, one more or less ingeniously turns the handle of a calculus. Again after the calculation is performed one interchanges the resulting non-ostensive with an ostensive statement. Indeed when I put one apple into my pocket and then another one and then assert that I have two apples in my pocket, I have, among other things, twice exchanged the ostensive relation of ' putting together ' with that of ' mathematical addition '. What is done almost automatically in counting apples, demands when first performed in a new field great care and creative imagination. The procedure is described by somebody who has been engaged on it in von Neumann and Morgenstern's *Theory of Games and Economic Behaviour* (chapter, I, section 1).

In order to discuss the nature of a typical interchange of ostensive with non-ostensive assertions, it is well to consider an accepted set of ostensive rules and the set of assertions which can be made in accordance with these rules to the effect that certain particulars are members of certain ostensive classes. We might decide to consider only this initial set of class-member assertions its logical consequences and the class-class relations between the ostensive classes. We disregard, in other words, the possibility of class-member assertions in accordance with ostensive rules other than those included in our original set. Is this decision a decision to treat certain ostensive particulars *as if* they only belonged to certain ostensive classes or is it a decision to replace ostensive particulars by a new kind of "abstract" particulars? The question is clearly no more than one of verbal propriety.

For some reason or other it may become desirable to take a further and more radical step, for instance to eliminate border-line cases. By an elimination of border-line cases I understand the replacement of a class-overlap by a class-inclusion or a class-exclusion. (This replacement must be distinguished from the replacement, discussed in the previous section, of an inclusion-or-overlap or exclusion-or-overlap by an inclusion, exclusion, or overlap.) Thus one may decide to replace the overlap between the ostensive classes of visible circles and visible ellipses by an exclusion. This decision implies firstly that the so related classes are no longer ostensive and secondly that the relation between the ostensive classes and that between the corresponding non-ostensive classes are in a peculiar way similar: the similarity is, of course, that between an exclusion and a small overlap, which is, so to speak, nearly an exclusion.

What shall we say about this decision to eliminate border-line cases? Two answers, at least, would seem plausible. One might, firstly, say that in making the decision one has created or constructed two classes of ideal particulars and that class-member assertions involving these apply to the corresponding non-ostensive particulars in virtue of the mentioned similarity between a small overlap, which is nearly an exclusion, and an exclusion. One might, secondly, say that the decision to ignore border-line cases does not involve the creation of ideal particulars, but merely a convenient simplification of a small overlap into an exclusion. These two answers are not really different. The first expresses the similarity between certain ideal and certain ostensive particulars in terms of the similarity between a class overlap and an exclusion, the second expresses the similarity

directly. It is the similarity between class-class relations which justifies the interchange of ostensive with non-ostensive statements in which the application of mathematics to experience frequently consists. That inferences from ostensive to ostensive assertions are frequently drawn by way of a double interchange of ostensive and non-ostensive class-class relations does not preclude the possibility of directly inferring ostensive from ostensive assertions. This way of directly inferring ostensive from ostensive statements can even be formalised by the construction of an algebra of indefinite classes on the lines indicated in the previous sections.

IV

Logicians are used to distinguishing what C. I. Lewis calls the "comprehension" and what he calls the "denotation" of a term (*An Analysis of Knowledge and Valuation*, p. 39). Thus, it is said that to the comprehension of "man" there belong all possible or consistently thinkable things to which this term would be applicable, while to its denotation there belong all actual things to which the term applies. I shall try to show that the distinction is spurious for ostensive predicates and that the impression of its being required by some perfectly intelligible ways of speaking arises from a misunderstanding of the nature of ostensive rules.

Ostensive predicates have no comprehension for the simple reason that, although one can think of ostensive particulars, one cannot think them and certainly cannot think them consistently. Nothing is consistently thinkable unless it is a meaningful sentence, a proposition, a judgment, a concept or in short, something that is capable of being a premise or a conclusion in a deductive argument. Whether non-ostensive predicates have a comprehension depends on what we understand by "a non-ostensive predicate" and thus, as has been noted, ultimately on the decision whether certain non-ostensive rules are "sufficiently" similar to ostensive ones.

The wrongness of the distinction between the comprehension and denotation of a predicate rules out an otherwise attractive theory according to which all logical relations between predicates are relations between their comprehensions and all factual relations between them relations between their denotations. Thus, on this theory entailment would be explained as a class-inclusion between two comprehensions and formal implication as an inclusion between two denotations.

It remains to discuss the kind of statements in which the occurrence of such words as "actual" or "existent" and of "conceivable" or "thinkable" seems to call for the distinction between the comprehension and the denotation of ostensive predicates. The following illustrations seem sufficient: (1) 'All conceivable men are (must be) vertebrates', (2) 'All actual men are featherless', (3) 'The class of all conceivable men includes the class of all actual men'. The first of these is interpreted as asserting that one comprehension is included in another; the second as asserting that one denotation is included in another. The third statement unlike the others would not normally be made. It would only be made under pressure by somebody who is committed to the false distinction between the comprehension and the denotation of ostensive predicates.

The intelligibility of the first two statements is not in any way dependent on the criticised distinction and is in keeping with what has been said about ostensive rules. The first statement means that whenever the ostensive predicate "man" is properly applied, *i.e.* applied in accordance with the ostensive rule guiding its use, then the ostensive predicate "vertebrate" is also properly applied. In other words, the ostensive class *man* itself, not a comprehension, is included in the ostensive class *vertebrate*. The second statement means that while the class-relation between *man* and *featherless* is inclusion-or-overlap no known exemplification of the former overlaps with any known exemplification of the latter. Even the rather unnatural third statement can be regarded as being compatible with the way in which ostensive predicates are being used. It may, somewhat charitably, be taken to mean that the ostensive class *man* is not given by any exemplification. This is indeed true of any ostensive class since any such class is given by an exemplification and a comparative clause which renders the exemplification incomplete.

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VII.—DISCUSSION

EMPIRICAL PROPOSITIONS AND HYPOTHETICAL STATEMENTS

I SHOULD like to add to Mr. Berlin's many-weaponed attack on Phenomenalism (in MIND, July, 1950) one further point, the importance of which seems to me to have been generally overlooked. This will not perhaps "refute" that resilient theory, but it will, I think be seen to necessitate yet another dilution of its claims, and in this way to hasten its eventual disappearance.

I

There is one preliminary matter to be settled. It seems to me rather undesirable—though not, for Mr. Berlin's case, importantly so—to say as he does that (some) categorical sentences "point" (to things, events, etc.). For such sentences out of context are like sign-posts lying on the ground in the middle of a field; we should not say that these sign-posts point to anything, but rather that they *could* do so—*i.e.* they would if they were put up in appropriate places. An unerected sign-post marked "Dunley 1½" does not, though it could be made to, point the way to Dunley; and a context-less sentence such as "This table is brown" is in a similar but even worse case, for we cannot even say what table it *could* be used to point to. It could in fact be used to point to any table; but it seems very queer to say that it actually does so.

If for these reasons we distinguish, in Mr. Strawson's way (MIND, July, 1950, p. 325), between a sentence (which could be used to point) and a use of a sentence (in order to point), part of Mr. Berlin's case can without difficulty be re-stated as follows: Some categorical sentences can be and are used to point to actual objects, states of affairs, etc.; but the chief characteristic of hypothetical sentences is that they are not, cannot be, are specifically not *meant* to be, used in this way. It follows that hypothetical sentences, and *a fortiori* hypothetical sentences about sense-data, cannot in every case be used to do the same job as is done by categorical sentences. Even if (as Professor Ayer holds[1] is never the case) our categorical and hypothetical sentences entail each other, substitution without loss is impossible.

It may be worth noticing that it may also be inadvisable to use the word "point". For it is true to say that I cannot literally point *at*, but only towards, or in the direction of, what is out of sight; and this seems to make it necessary to give a special explanation of the use of sentences to "point" to objects in absence. Mr. Berlin

[1] *Foundations of Empirical Knowledge*, pp. 239-241.

describes this use of "point" as "semi-metaphorical". But his use of "point" is wholly metaphorical in any case; and might we not avoid some unnecessary difficulties by substituting for "point" some other expression, the *literal* use of which implies no restriction to present, visible objects?

II

To return now to the distinction between a sentence and the use of a sentence, and the point of making it.

Mr. Berlin indicates the phenomenalist's programme in (among others) the following ways (p. 291): (1) he offers an "analysis of how material object sentences are to be 'reduced' (without residue) to sentences describing both what the observer does, or did, or will observe, as well as what he would, or would have, might or might have, observed under appropriate conditions": and (2) he proposes "the reduction of material object sentences into what we may, for short, call sense-datum sentences". These are, I think, fair descriptions. And we may add a version from Professor Ayer: "What is being claimed is simply that the propositions which are ordinarily expressed by sentences which refer to material things could also be expressed by sentences which referred exclusively to sense-data". (*Foundations*, p. 232.) This version is slightly complicated by the use in it of "proposition"; but if we take it that Professor Ayer is here using "proposition" as, roughly, a way of referring to a class of sentences having the same meaning, we may re-write his claim thus: "For any sentence which can be used to refer to a material thing, there is a set of sentences having the same meaning which can be used to refer exclusively to sense-data". This is no doubt what is meant by saying that Phenomenalism offers an "alternative language".

It seems to me that Mr. Berlin's two versions, and Professor Ayer's version, reveal even in this (already diluted) form of Phenomenalism a special absurdity, which although not recondite has not, I think, been previously emphasised; and which can be avoided only at the cost of still further and possibly fatal dilution.

It must be noticed that the claim is to analyse (reduce, translate) *sentences* (or propositions = classes of sentences). Let us take, then, the sentence "This table is brown". Now, so long as it is the sentence that we are considering, and not some particular use of it by a particular person, the questions "What table is being referred to?", "By whom?", and "In what circumstances?", are not in point. This sentence *could* be used, by some person or persons, to refer to some particular table in some particular circumstances; but the unused, out-of-context sentence specifies no speaker or writer, no table, and no situation. Suppose then that we invite a phenomenalist to analyse (reduce, translate) the material object sentence "This table is brown": his reply must inevitably take

the following form—"If an observer [who ?] were to do something or other [what ?], or were in some situation or other [which ?], there would occur some sense-data [which ?]". Now it is perfectly clear that it would be no good trying to answer the bracketed questions. For as soon as we seek to specify who is to do what, or how he is to be situated, to achieve what result, we require information about some particular occasion on which the material object sentence may be supposed to be used; and thereafter we are not analysing (etc.) the *sentence*, but making possibly true statements about the circumstances of some one occasion of its use; and these possibly true statements will certainly be false of almost every other such occasion. But at the same time it is no good refusing to answer them; for unless we do so, we cannot even begin to compose sense-datum sentences; we can only hint that, in rather different circumstances, we could have done so.

A parallel absurdity can be seen inevitably to arise out of attempts to reduce, in this sense, even sentences offering better *prima facie* opportunities for this treatment. It would, for example, be generally admitted that an infantry battalion is composed of men. But consider the sentence "The battalion is on a night exercise". It is clearly impossible to maintain that any set of sentences, containing the proper names of individual men, means the same as this sentence. For by inspection of the sentence alone, we do not discover what battalion is in question, at what stage of its history, nor which of its members are supposed to be present, nor what they are supposed to be doing. Of course, given some particular occasion on which this sentence was used, we might laboriously attempt to compile some list of sentences, using the proper names of individuals, and describing their activities. But it is clear that the statements made by the use of this list of sentences would almost certainly be false, if made on any other occasion of use of the sentence "The battalion is on a night exercise"—even if the battalion referred to happened to be the same battalion. Thus, unless we are prepared to adopt the desperate and fantastic view that the meaning of the sentence "The battalion is on a night exercise" is (or is almost certain to be) different on each occasion of the use of this sentence, and remains quite indefinite until some such occasion is specified, it is necessary to conclude that the sentence is not susceptible of analysis in the way proposed.

The root of the matter can be revealed in this way. Any list of proper names, however long, supplies us with a means of referring to *some* particular persons, however numerous; whereas it is the chief (and of course the intended) merit of such expressions as "the battalion" that they do *not* do this. Of course the doings and undergoings to which I may refer by statements about the battalion are in fact the doings and undergoings of individuals; but by using the expression "the battalion" I do not say *which* individuals, and it is for precisely this reason that sentences in which this expression occurs can be used on many very different occasions. The respects in which

such occasions must be broadly of the same *sort*, in order to allow correct uses of the expression " the battalion ", or of such sentences as " The battalion is on a night exercise "—these are the very respects which *cannot* be brought out by the compiling of any list of sentences containing the proper names of individuals and descriptions of their doings. In just the same way, any list of sentences mentioning the particular conditions in which certain sense-data are or could be had by somebody, can at best supply us only with the means of referring to some particular occasion of use of such a sentence as " This table is brown ". The *sort* of situation in which the sentence may be used, the very general respects in which each situation must resemble at least some of the others—this (which is what we know when we know the *meaning* of the sentence) is precisely what cannot be indicated by any list, however vast, of fatally specific, particularising sense-datum sentences.

From the hopeless attempt to analyse the sentence " The battalion is on a night exercise ", we can gather only that battalions consist of men: and from the even more hopeless attempt to analyse the sentence " This table is brown ", we can gather only that tables are visible.

III

It seems clear that for these reasons, which I think are different from and additional to Mr. Berlin's, the phenomenalist's claim to provide a general analysis of material object sentences must be disallowed; and historically it is certain that this claim has been advanced, and not in any trivial, unimportant way that could plausibly be ascribed to mere terminological carelessness. However, it might I suppose, be retracted, and replaced by the less ambitious claim to provide, whenever a material object sentence is actually used, some substitutable set of sense-datum sentences—these sets being admittedly very different from each other even where, on the various occasions on which they are offered, the very same material object sentence is used. No doubt there is a surprising number of further reasons for rejecting even this more modest claim; but even if it could be allowed, much orthodox phenomenalist doctrine would still be objectionable. In particular, it would be highly misleading to maintain that Phenomenalism offers an " alternative language ", or some sort of translation. For in the normal, honest use of " translate " it is possible to translate accurately " This table is brown " without knowing which table, when, where, and referred to by whom. But the fact is that, in a " sense-datum language ", there would be nothing to which we could point as *the* translation of " The table is brown "; for no collection of analyses of particular uses of this sentence would mean the same as the sentence itself. There is thus no genuine inter-translateability. This is similar to the point which Mr. Berlin expresses materially by saying (p. 296) that Phenomenalism offers " a different picture of the world

. . . and in no sense merely a description of the old picture". If "alternative" language means "totally different" language, very well; but surely the suggestion has usually been that the new language would be not only similar, but just as good or better—even, it has been said, for some purposes more convenient. This last is perhaps the oddest suggestion of all.

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CONCERNING THE DEFINITION OF CLASSES.

ONE of the motives that led philosophers to distinguish between a *property* and the *class* of all individuals that possess this property (briefly: the corresponding class) can *intuitively* be stated as follows: It does not seem evident that everything that can be truly predicated of a class can also be predicated of every property to which this class corresponds. Within the symbolism of *Principia Mathematica* (supplemented by constant predicates and, for purposes of exposition, limited to the first two predicate types) this idea has found the following realization.

1. The predicate symbols are interpreted as properties.

2. The formula

$$(f(x) \equiv_x g(x)) \supset (f = g),$$

expressing extensionality, is neither a primitive sentence nor a theorem.

3. Classes are introduced as so-called incomplete symbols by first writing

(1) ' $F(\hat{z}(fz))$ ' for ' $(\exists g)\,[(g(x) \equiv_x f(x)) \,.\, F(g)]$ '

and then proving

(2) $$(f(x) \equiv_x g(x)) \supset (\hat{z}(fz) = \hat{z}(gz)).$$

For consider now three constant predicates, ' ϕ ', ' ψ ', ' Ψ ', of types (0), (0), ((0)) respectively and assume the three sentences

(3) $$\phi(x) \equiv_x \psi(x), \quad \Psi(\phi), \quad \sim \Psi(\psi)$$

to be true. It then follows from (1) that the two equivalent sentences

(4) $$\Psi(\hat{z}(\phi z)), \qquad \Psi(\hat{z}(\psi z))$$

are also true. In words, what can be truly predicated of the class and of one of two properties to which it corresponds cannot be so predicated of the other. Thus, Russell's procedure realizes the intuitive idea with which I started. One sees easily that it also leads to the following result.

R. Everything that can be truly predicated of a property can also be so predicated of the corresponding class. In symbols, ' $F(f) \supset F(\hat{z}(fz))$ ' is a theorem.

Carnap [1] has recently proposed to modify Russell's procedure by writing, instead of (1),

(1') ' $F(\hat{z}(fz))$ ' for ' $(g)\,[\,(f(x) \equiv_x g(x)) \supset F(g)\,]$ '.

Since (2) can again be proved, this proposal realizes part of the intuitive idea of class. Instead of *R*, however, one obtains now

C. Everything that can be truly predicated of a class can also be so predicated of every property to which it corresponds. In symbols, ' $F(\hat{z}(fz)) \supset F(f)$ ' is a theorem.

[1] *Meaning and Necessity*, (1946), pp. 147-150.

In view of C it appears that, whatever other advantages it may have, Carnap's proposal does not realize one of the intuitive ideas that have led to the distinction between properties and classes. The only advantage actually claimed for (1′) is that it avoids the following consequences of (1). If the sentences (3) are assumed to be true, then the two sentences

$$[\hat{z}(\phi z)] \sim \Psi(\hat{z}(\phi z)), \quad \sim [\hat{z}(\phi z)]\, \Psi(\hat{z}(\phi z))$$

have opposite truth values. However, it could be said that this supposedly undesirable feature of (1) merely illustrates the importance, recently emphasized by Smullyan,[1] which the scope of incomplete symbols has in all nonextensional contexts.

Perhaps it is also worth noticing that if one assumes again the sentences (3) to be true, the sentences (4) are, upon Carnap's proposal, both false. In words, no context not extensional with respect to a property can be truly predicated of the corresponding class. In a sense this trivializes proposal (1′). As one would expect, for extensional contexts both (1) and (1′) yield '$\Psi(\phi) \equiv \Psi(\hat{z}(\phi z))$.'

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[1] "Modality and description," *The Journal of Symbolic Logic*, 13 (1948), pp. 31-37.

IS "YOU CAN'T FOOL ALL OF THE PEOPLE ALL OF THE TIME" AN ANALYTICAL PROPOSITION?

An *obiter dictum* of Mr. J. L. Austin in the course of his valuable paper on "Other Minds" (*Proceedings of the Aristotelian Society*, Supp. Vol. XX (1946), p. 184 n.) states that "You can't fool all of the people all of the time" is an analytic proposition. Now this is at first sight surprising; undoubtedly Abraham Lincoln and those who have repeated the remark since have thought they were stating an empirical fact about the possibilities of deceiving human beings, something we should like to believe true, though we may think there are good grounds for doubting it in that a great many people are fooled a great deal of the time. But if the proposition is analytic it is emptied of all factual content. I hope to show first that circumstances might arise which would make it probable that the proposition was false, in which case, even if true, it will be empirical and not analytic; and secondly that the problems involved present close similarities to ones arising in the investigation of telepathy and/or clairvoyance.

In the first place, Mr. Austin's position appears to depend on the verification theory of meaning, at any rate as applied to empirical propositions. If so, we may suppose it to run somewhat as follows: "'You can't fool all of the people all of the time' cannot be an empirical proposition, for it could never be falsified. The moment you show that some one has fooled all of the people all of the time he has ceased to fool you, and so the discovery of an instance where it appears true to say 'Every one has been fooled about x' automatically makes the assertion false in future; *i.e.* no general proposition of the form 'Every one is always fooled about x' can be verified so as to falsify the proposition 'You can't fool all of the people all of the time.'" Now it is plain that if we question the verification theory of meaning this argument falls to the ground, but the conclusion still need not follow if we accept it.

From a demonstration that some one has fooled all of the people all of the time, it does not follow that he has ceased to fool you; that follows only where what is shown is not merely that he has caused deception, but what the false proposition is. The former may occur without the latter in more ways than one. We may know that of a series of propositions p, q, r, s . . . one is false and the rest true, without being able to say which is false, and it is perfectly possible that no one in the future will know either. The position is similar where we know any determinate or indeterminate number of propositions forming less than half of the series to be false, but these further cases do not need separate elucidation, and I shall for the sake of simplicity confine myself to the first. Here, then, we shall not know which of the series p, q, r, s . . . to doubt, and the

longer the series the smaller the probability that any particular member, say p, is the false one. This probability will range from ·5 where the number of propositions in the series in 2, to 0 where it is ∞; thus wherever the series contains more than two members it will be rational for us to accept p on the balance of probabilities, the more so the longer the series, and so we shall continue to be fooled at some one point. We may therefore know that X, in asserting a series of propositions, has fooled all of the people all of the time, and still be fooled by him in respect of one of them, where the series contains at least three members.

The analogous problem in the investigation of telepathy and/or clairvoyance is as follows. How can we test whether there are cases of clairvoyance which are not explicable as cases of telepathy, if once we admit the possibility that telepathy may be precognitive? For if we admit that possibility, what is claimed as an instance of clairvoyance may be explained as telepathic precognition of the state of mind of the verifier at the time of verification, whether the verifier be the original experient or some other; and so the only possible instances of clairvoyance which cannot be explained as telepathic will be those that never receive verification and cannot be known to be such. The solution is that if I make a number of assertions whose truth I claim to know by clairvoyance alone, and could know only by clairvoyance or such precognitive telepathy, and what is verified afterwards is not the truth of my individual assertions but the total number of cases in which my assertions have been true, then no individual asserted instance of clairvoyance will have been a possible instance of precognitive telepathy, and so the question "Is there pure clairvoyance?" will be decidable on empirical grounds. Apparatus for the purpose has, in fact, been constructed: see D. Parsons, *Attempts to Detect Clairvoyance and Telepathy with a Mechanical Device* (*Proceedings of the Society for Psychical Research*, Vol. xlviii (1945-9), pp. 28-32).

It is inconsistent to assert (i) "p is false", (ii) "I believe p" (likewise "Everybody believes p", since "everybody" includes the speaker), and similarly to assert (i) "p has a probability greater than $\frac{1}{2}$ but less than 1", (ii) "I believe (adhere to) p unconditionally (unreservedly)." So far we may agree with Mr. Austin. But there is no inconsistency in asserting (i) "p has a probability greater than $\frac{1}{2}$ but less than 1", (ii) "I believe (adhere to) p provisionally"; and this involves the reasonableness of acting on p. It is this latter case, and this provisional belief, that are in question when, of propositions $p, q, r, s \ldots$, one is known to be false, but which is false is uncertain.

Another possibility is the following. Circumstances are imaginable in which I may know that X is guilty of universal deception, but not what the deception is, even if I know him not to have asserted more than two propositions, or even one. He may have asserted the false proposition p, and everybody may believe it on his authority,

directly or indirectly. If I now learn that X has asserted a false proposition "p", I shall have the same (or almost the same) justification for believing p as before, if I do not know that "p" is p (in this instance, if X is only indirectly my authority for believing p, and I have no reason for thinking him the original authority of my informant).

It is in a situation vastly more complicated than those discussed here, but exemplifying their characteristics, that we find outselves all our lives.

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D. COX: THE SIGNIFICANCE OF CHRISTIANITY: A NOTE

CHRISTIAN doctrine has in the past suffered from too much abstraction on the one hand and the accretion of fanciful elements on the other. Because of this many will welcome Mr. Cox's article (MIND, April, 1950) as an attempt to make that doctrine more intelligible to the layman. Nevertheless, I find that some difficulties arise from his general thesis, and they may be summed up in the statement that the verification principle is not adequate when applied to the fundamental propositions of Christian teaching. Mr. Cox nowhere indicates how he would regard doctrines that are unique to Christianity, but I am assuming that with regard to the basic doctrines his belief is orthodox.

One of the fundamental Christian beliefs is that in Christ God was incarnate, and because God is a unity, then He was in Christ in all His fulness; He is not merely the Son of God, but God the Son, the second Person of the Trinity. Christ, then, was someone quite unique in that He was both God and man. If Mr. Cox grants this doctrine, it seems to me that he gets into difficulties when he applies his verification principle to test the significance of the statements of Christ. According to his version of the verification principle there are two types of significant statement, *viz.* empirical hypotheses, which are statements that can be verified by *human* experience, and formal rules, which indicate how we are to use theological terminology. Under which of these types of statement are we to place the utterances of Christ? Many of these are, no doubt, empirical hypotheses, but there are some which are more difficult to classify. For example, how would Mr. Cox regard such a statement as "I and my Father are one"? It seems to me that this remark is not verifiable by human experience, that is, it is not an empirical hypothesis. Christ in this instance is not speaking *qua* man but *qua* God. Some people might argue that mystics make similar claims of oneness with God, and that there is therefore no necessity for us to believe that Christ's statement is based on an experience any different from theirs. Such a view ignores completely the doctrine which we are assuming to be held by Mr. Cox, *viz.*, the divinity of Christ. It seems to me that the Christian must believe that when Christ speaks about His relationship with the Father, He is speaking about a relationship that is similar to no other. His statement, then, is not an empirical hypothesis. Nor is it a formal rule. Formal rules, according to Mr. Cox, "indicate in what ways the terms of theological jargon are to be used". The statement obviously does not do that. Can it be argued that it resembles statements of the type which, *e.g.* assert that Sir Walter Scott and the author of *The Fair Maid of Perth* are one and the same person? No, because while there is but one

God, yet there are three Persons in the Godhead. So that Christ and the Father are two Persons, and the statement "I and the Father are One" can be regarded as asserting something worthwhile and very important. If the statement under consideration is neither an empirical hypothesis nor a formal rule, what is it? It seems to me that the only solution is to regard it as metaphysical, *i.e.* as a statement which has significance, and yet is not verifiable by human experience. Would Mr. Cox be willing to accept this solution?

My second objection is more serious. Mr. Cox states that the Church formulated her doctrines in order to safeguard Christian experience. (How does one "safeguard" Christian experience?) The functions of such doctrines are (1) laying down formal rules for the use of theological terminology, and (2) indicating the possibility or the probability of the occurrence of certain experiences. As an example of what he means Mr. Cox chooses the central doctrine "God exists". (As this doctrine is basic I shall confine my remarks to it.) First, we require a formal rule which will indicate how we are to use the word "God". The formal rule will describe such experiences as "meeting God", "encountering God," or "knowing God". When this is done we can translate the doctrine "God exists" into:

"Some people have had, and all may have, experiences called 'meeting God'."

Mr Cox would maintain that the essential content of the doctrine is unaffected by his reformulation. This I wish to deny. Logical positivism and theism are incompatible, and when one is applied to the other, the metaphysical element, that is an integral part of theism, is lost. Let us consider the phrase "meeting God". Mr. Cox suggests that "the rules governing the use of the word 'God' will be rules which describe the human experiences which are to be called such names as these I have mentioned ['meeting God', 'knowing God', 'encountering God']". In other words, according to this argument, 'God' is a word for a particular type of human experience and nothing more. So that when we say that He exists we are saying that such experiences have occurred in the past, and there is the possibility that they may occur again. This seems to me to be altogether wrong. Let us consider the experience of the mystic —the most striking example of the experience 'meeting God'. Mysticism, as Professor Rufus Jones says, "expresses that type of religion which puts the emphasis on immediate awareness of relation with God, on direct and immediate consciousness of the Divine Presence". For the mystic, then, God is more than a word for a particularly rare kind of experience, but an entity which continues to exist even when the experience has ceased to exist. The mystic has direct knowledge of this reality, so that there are just two terms involved, himself and God, and God's continued existence apart from himself is as indisputable to him as the objects of the material world

are to the ordinary man. To translate the doctrine of God's existence into experiential terms is completely to misunderstand the mystics.

It is possible that some who have the experience which Mr. Cox calls "meeting God" do think of it on the analogy of sensation, *i.e.* as not giving direct knowledge of God's existence. Two courses are open to such people.

(*a*) They might try to explain these experiences in terms of mental and bodily conditions, maintaining that there is no necessity to bring in a supersensible reality. That is, if you could destroy all the people who have had or who might have this type of experience then you would destroy God. This course is open to the Christian positivist, who might still claim to have the experience "meeting God". No comment is needed.

(*b*) They might feel that such experience must be explained as the effect of a cause that can only be God. Here, the existence of God is put forward as a hypothesis, and I venture to say that this is how most thoughtful Christians, who have not had mystical experience, regard the proposition "God exists". They find God is necessary to explain not only mystical experience, but also the existence of themselves and the universe : He is "the Maker and Preserver of all things both visible and invisible". Here, too, God is obviously regarded as continuing to exist even when the experience has ceased to exist. God is more than a human experience.

To sum up :

(*a*) We might accept the verification principle, but in doing so we should have to reject doctrines that are unique to Christianity. What, then, is the point of talking about the significance of *Christianity?*

(*b*) If the basic doctrines of theism are accepted then the verification principle is not effectively applicable to them. The "essential content" of the doctrines is lost.

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VIII.—CRITICAL NOTICE

Knowledge and Perception. Essays and Lectures by H. A. PRICHARD. Edited by SIR DAVID ROSS. Clarendon Press, 1950. Pp. ii + 214. 15s.

PRICHARD was best known to the outside world as a moral philosopher. But in the eyes of his friends, colleagues and pupils he was first and foremost an epistemologist, and a most acute and original one. (Indeed much of his moral philosophy is epistemological too.) The only book he published was *Kant's Theory of Knowledge* (1912) and some half-dozen articles of his on the theory of knowledge appeared in MIND. But he came to be dissatisfied with his earlier views, especially on perception, and was never wholly satisfied with his later ones. During the last thirty years of his life, 1918-1948, he published very little on the theory of knowledge, though he thought and wrote about it a good deal. The result was that his epistemological views were almost unknown to his younger contemporaries, even in Oxford. They knew that he *had* views on the subject because of his occasional interventions, characteristically laconic and explosive, at philosophical meetings. They knew too that he strongly disapproved of almost all the current theories. But the positive doctrine, which formed the basis of those disapprovals, was never revealed to the world. It is revealed in this book.

The reader must not think that it is just another book of 'collected papers' which he may be excused from studying with care, because he has read them all before. A good three-quarters of the book has never appeared in print; and the two papers which have, 'Mr. Bertrand Russell on our Knowledge of the External World' (1915) and 'The Sense-datum Fallacy' (1938), are much more easily intelligible when read in the context of the others. If any potential reader is in doubt about the value of the book, I should advise him to begin with the two short papers, No. 2 'Seeing Movements' and No. 3 'The Apprehension of Time'. They are only eleven pages in all. They certainly demand 'close reading', to use a favourite phrase of Prichard's own. But they are very characteristic samples of Prichard's thought, and I believe the reader will find them an excellent eleven pages' worth.

It goes without saying that the editor, Sir David Ross, has done his work most admirably. I have noticed only one misprint, p. 151, par. 4, 'independence of' for 'dependence on'. He mentions in the Preface certain papers which he has *not* included, with dates and references. (I confess I am a little sorry that he has not reprinted the one called 'A Criticism of the Psychologists' Treatment of Knowledge', MIND, 1907.) He also mentions Prichard's philosophical letters. All those who had the privilege of receiving them—

and the labour of trying to answer them as they deserved—will agree with Sir David Ross's estimate of them : that they often 'exhibit the firmness and subtlety of [Prichard's] thought as forcibly as anything that he published or that has been published since his death '. It is regettable that they are not in a form suitable for publication ; and one is glad to know that many of them are to be deposited in the Bodleian Library.

The first and earliest paper in the book, ' Mr. Bertrand Russell on our Knowledge of the External World ' (MIND, 1915), is a minute and very severe criticism of Lord Russell's Lowell Lectures, and of his essay 'The Relation of Sense-data to Physics', originally published in the periodical *Scientia* and subsequently in *Mysticism and Logic*. At that time Prichard still held the more or less common-sensical theory of perception which he had expounded in his own book, *Kant's Theory of Knowledge*. For that very reason, perhaps, some present-day Linguistic Philosophers will find this paper particularly instructive, since they have recently returned to something very like the common-sensical theory which Prichard started from, and subsequently abandoned. For example, we find him maintaining that the object of perception is always a Body (I write the word with a capital because in Prichard it is almost a technical term), and that if one did not already know what the word ' Body ' means, one could not know what the word ' appearance ' means either (p. 31). Thus if we are directly aware of appearances, as Russell says we are, we must, *ipso facto*, be directly aware of Bodies ; and it must be nonsense to speak of any process by which we ' advance ' from knowledge of appearances to knowledge of Bodies. Moreover, no group or class of appearances can be a substitute for a Body, as Russell maintains that it is when he says that a thing is a logical construction out of what are commonly called the appearances of it. For the unity of such a group of appearances arises simply from the fact that they are all appearances of the same Body. ' Whether the view which Mr. Russell takes to be the true view which is to replace that of common sense is true or not, it is impossible even to *state* it without falling back on the language of common sense, and therefore without presupposing the truth of the thought which underlies this language ' (p. 38). These words should rejoice the heart of any present-day Linguistic Philosopher.

It is true that in his later writings Prichard was much less common-sensical, as we shall see. But even when he came to think that we do not perceive Bodies, he still refused to say that we do perceive appearances ; on the ground that we should in effect be claiming to perceive Bodies after all, since by ' an appearance ' we can only mean an appearance *of* some Body.

What of the notion of an unsensed appearance ? Russell's classes of appearances, it will be recalled, always contain unsensed members ; and some of them—those which we call ' totally unobserved objects '—contain nothing else. On this Prichard's

comment is that 'appearance' is a *doubly* relative term: an appearance is *to* someone as well as *of* some Body. The notion of an unsensed appearance is therefore just absurd (p. 31, *cf.* p. 9). I confess I do not know what Russell's answer is to this very obvious objection. I suppose it is that an appearance is relative not to a percipient, but to a point of view; or to a perspective, since every appearance, sensed or not, is a member of a perspective.

Among all the paradoxes which Prichard found in Russell's theory (and he could find little else) Russell's conception of Space appeared to him the most paradoxical. It seemed to Prichard self-evident that there is only one Space, Space with a capital S, and that it is Euclidean. Now of course Russell's whole theory of the external world is unintelligible if one can make no sense of the distinction he draws between Private Spaces and Public Space, the Space of Points of View. And Prichard could make no sense of it at all. This prevented him from appreciating the most original and ingenious feature of Russell's theory, the contention that there are *unsensed* private spaces or private worlds. For every perspective, sensed or not, is according to Russell a private world; private not to a percipient, but to the point of view from which alone it exists. On this question, I can only say that to me it does not seem self-evident that there can only be one space, nor that it must be Euclidean; though no doubt the facts which are described in a terminology of Private Spaces could also be described in quite a different one. Prichard, in this stage of his thought, would have described them in a terminology of 'Appearing'. At a later stage of his thought, when he had become less common-sensical, he described them by saying that two colours (*i.e.* colour-expanses) could be in the same place, and that a colour could be in a place which is also occupied by a Body: for example, when I look at my face in a mirror, the colour which I see is in the same place as the wall behind the mirror.

It is not surprising that Prichard in 1915 found the doctrine of Logical Constructions extremely puzzling. Some people are a good deal puzzled about it to this day. He takes Russell to be maintaining that the statements of common sense and of science can be known to be true if, but only if, certain crucial terms in them (*e.g.* 'body', 'change', 'place') are given *new meanings*; that is, if new concepts, which are known to have application, are *substituted* for certain fundamental concepts of common sense and science. 'Thus the common-sense statement "my pen dropped on the floor" will be true if, though only if, it means "a class of appearances dropped on another class of appearances". And "bodies move" will be true if, though only if, it means "certain classes of appearances move"' (pp. 28-29). But he objects, the statements if so interpreted are certainly false. 'In fact Mr. Russell's view is exposed to the familiar objection which Berkeley quotes against himself . . . "it sounds very harsh to say, we eat and drink ideas, and are clothed with ideas"' (p. 29). Again, if things are those

series of aspects which obey the laws of physics, as Russell says elsewhere, 'this *must* mean that it is true to say of certain series of aspects that action and reaction are equal and opposite, or that they attract one another inversely as the square of the distance' (p. 30). Already in 1915 Prichard sees that there might be an answer to this objection. We might give new meanings to words, like 'attract' and 'action' also. But then, he says, 'if this process is fully carried out we shall be left with nothing but language appropriate to Berkeley, with no justification for retaining *any* of the language of common sense and science'. And in that case 'Mr. Russell's whole mission will have gone' (p. 30).

There are two comments to make on this. First, even at the worst the new language would not be altogether Berkeleyan, because we should constantly have to refer to unsensed appearances, unsensed perspectives, and unoccupied points of view. Secondly, I think Prichard has misunderstood what Russell's mission was. Russell wanted to maintain that the statements of common sense and science are in *some* sense true, and to show in what sense they are so. In showing this, why should he not translate the verbs (as well as the nouns) which are used in ordinary or in scientific language, and moreover the syntax too; provided that the new statements, despite their new vocabulary and syntax, are still equivalent to the old? But if this was Russell's mission, it must be admitted that he was not always clear about it himself. As Prichard complains (p. 7, n. 1) Russell sometimes speaks as if common sense was a kind of theory, an ingenious but false *philosophy* invented by 'Stone Age metaphysicians'. In that case, the statements of common sense (and, I suppose, the statements of science too) would have to be denied, not merely clarified or analysed, and new statements substituted for them which do not leave these erroneous metaphysical implications.

The next three papers in the book, the two short ones on 'Seeing Movements' and 'The Apprehension of Time' and the long one on 'Perception', may be considered together. The two short papers are dated by the editor '*c.* 1925'. The long one, a paper read to a Cambridge Society, is undated. I should guess that it was written in the early 1930's. At any rate, all these belong to the later stage of Prichard's thought, when he had abandoned the commonsensical theory of perception presupposed in his criticism of Russell. In these three papers we see him working out a new theory and giving his reasons for abandoning the old one. The new theory is as queer as it is ingenious, and Prichard was himself conscious of its queerness. He was driven into it most reluctantly, and I do not think he was ever wholly satisfied with it. But however he may have felt about it, it is well worth examination.

The question from which he started was quite simply 'What do we perceive?' and especially 'What do we see?' We have an almost

irresistible inclination to answer 'Bodies', and this was the answer Prichard himself had given in his earlier days. But now let us consider, for example, the perception of movements. We must first distinguish between seeing something move, and thinking or judging that it has moved (p. 41). We *judge* that the hour hand of a clock must have moved, because we see it pointing to XI at one time and to XII at a later time. But we *see* the second hand move; at least we should all say so before we reflect. But do we really see it moving? We really are seeing, and not judging; we must stick to that. But do we really see a certain Body moving? Suppose we are in a lift, going upwards. We *see* the walls of the lift-shaft moving downwards. But they are not in fact moving downwards. It is we who are moving, and not the walls.

The reader might object that after all we do see a real *relative* movement. Oddly enough, Prichard does not consider this objection. The reason is that in his view there is no such thing as relative motion. As he makes clear elsewhere, he thinks it self-evident that all motion is absolute; anyone who holds that there is relative motion must be confusing motion with something else, namely alteration of distance, and relative motion is a contradiction in terms (*cf.* p. 59, *ad fin.*). But this surprising contention is not, I think, essential to Prichard's present argument. I think we must admit that *perceived* motion is absolute, whatever may be the case about physical motion. Moreover, there are other illusory perceptions of movement which have nothing to do with relative motion: for example, the illusory movement of an illuminated spot in certain gestalt-experiments. It is sufficient for Prichard's purpose if there are illusions of movement at all. Now if there are, it follows, according to Prichard, that we *never* see Bodies move; and we never see them at rest either. But worse than that, it follows that we never see Bodies at all. For, as he says, 'we cannot separate the "bodily-ness" of what is seen from the moving of what is seen' (p. 43).

The thing really seen, then, is something other than a Body moving. What can it be? We cannot answer 'certain appearances'. For Prichard still insists that 'the term appearances is only appropriate so long as we presuppose that we do see Bodies' (p. 44, *cf.* his criticisms of Russell, above). We must therefore say that what we see is 'certain colours' or 'certain spatially related colours'. And we must say that we *mistake* these colours for a moving Body. Moreover, 'the very question "What do we really see when we are said to see a Body moving?" involves a fallacy—the fallacy of thinking that to see something is to *know* or *apprehend* it in a particular way' (p. 45, my italics). In other words, to alter Prichard's terminology slightly, having sensations is *not* a form of knowing, and our ordinary everyday perceptual consciousness *is* a form of mistaking. These are the two central theses of Prichard's later theory. And they lead him to reject not only common-sense Realism, but also the

sense-datum theory which various philosophers, from Locke onwards, have tried to substitute for it.

In the second short paper on 'The Apprehension of Time', Prichard begins by discussing what other people would call the Specious Present, though he carefully abstains from using that phrase. If every sound has a finite duration, how can we ever *hear* a sound at all? To hear is to hear at a moment: and 'what we apprehend by way of hearing . . . can only exist at the moment of hearing' [why is this so obvious?]. And by the time that a moment comes, part of the sound is over; indeed, strictly speaking, it is all over (p. 47). Kant's solution of the difficulty, in the passage about the Three Syntheses, does not help us. It 'makes *hearing* a succession of notes, A, B . . . into what we distinguish from that is coming to *think* of AB as successive' (p. 48). In any case, there is a succession within A itself and within B likewise, and how is that to be dealt with? Our provisional conclusion, so far, must be that 'perceiving anything which has a duration is absolutely ultimate and cannot be resolved into anything else.' But we still have not explained how we can perceive something which at the moment of perceiving no longer exists. 'We seem equally to have to maintain that we do hear sounds and that we do not' (pp. 48-49).

I do not altogether understand Prichard's solution of this dilemma. The task is not made easier by the fact that the rest of the paper is almost wholly concerned not with hearing but with seeing. Somehow or other the two doctrines I mentioned just now—that perception involves mistaking and that seeing, hearing, etc. are not forms of knowing—are supposed to provide the solution. I think he is maintaining that as we do not really see Bodies, so we do not really hear sounds either, if we mean by 'sounds' (as we ordinarily do) processes occurring *in rerum naturâ*, independent of our hearing them. Therefore—I suppose—the problem, of explaining how we can perceive as a whole a process whose earlier parts have by then ceased to exist, will no longer arise, because there was no such process: we were only under the mistaken impression that there was.

It might be objected that even if the *esse* of sounds is *percipi* (or involves *percipi*, as Prichard preferred to say) they have duration still, whereas the *percipere* is momentary. But I think that Prichard, in this paper, is intending to put his theory in an even more radical form than before. In the previous paper he did at least allow that we see colours, though we mistake them for Bodies, and that we hear sounds, though we mistake them for physical processes independent of ourselves. But I think he now wants to maintain that the word 'see' *means* 'to apprehend a Body in a certain way'; and since we do not in fact apprehend Bodies in that way, he draws the conclusion that we do not really see at all. We only *seem* to see—*i.e.* are in a state which we take, by a kind of illusion, to be seeing, though in fact we are not seeing. By a similar argument, presumably, we should have to conclude that we do not hear either. We only

seem to hear. But of course the difficulties about the Specious Present only arise on the assumption that we do sometimes see and hear. If we never do, but only seem to, *cadit quaestio*.

If this doctrine was really intended to solve the problems of the Specious Present, I can only comment that the cure is worse than the disease. For it involves us in maintaining (as Hume put it) that 'when we are most intimately conscious we may be mistaken'. If I cannot be certain that I do sometimes hear or see, what can I be certain of? At any rate, for good or ill, Prichard did not stick to this queer theory. In his later writings he returns to the original and less radical version of his doctrine, which does at least allow us to say that we really do see colours and hear sounds, even though it also insists that we habitually mistake them for Bodies and physical processes.

We may now turn to the paper on 'Perception', perhaps the most interesting in the book. Prichard begins as before, by asking 'What do we see or touch?' The unsophisticated answer is 'Bodies'. But any illusion of sight or touch is sufficient to show that this answer is false. Indeed, any situation—whether ordinarily called an illusion or not—in which an object (as we say) looks other than it is would be sufficient, as when a sloping field on a distant hill looks vertical. It is impossible for a Body to look other than it is (pp. 53-54). What then *do* we see? Colours, *i.e.* extended colours which are spatially—and also temporally—related. And the space they are extended and located in is not a visual space or a private space; these phrases are only 'words to which no thought corresponds'. The space in which colours are is the only space there is, 'that one space parts of which are occupied by Bodies, if there are Bodies' (p. 56).

But in saying this are we not attributing to colours properties which belong only to Bodies? And if we ask the question 'what do we really see?' with the assumption that we do *not* see Bodies, is the question still legitimate? (p. 57). For example, if someone asked us 'Did you really see Jones?' we should allow if pressed (a favourite Prichardian phrase) that all we really saw was the back of a man, and that we only thought or judged or had the impression that it was the back of Jones. But here it is implied that what we really saw was the coloured surface of a Body; and this applies to all ordinary uses of the antithesis between 'what I really saw' and 'what I thought it was'. And if we now proceed to deny that we ever see the coloured surface of a Body, and *then* put the question 'what do we really see?' are we not asking it just once too often? (pp. 57-58). Moreover, if I really only see colours and mistake them for Bodies, is it not a very queer kind of mistake, quite unlike mistaking the back of another man for the back of Jones? If I see a Y and mistake it for an X, I must be at least capable of thinking or judging a Y to be an X. It must be at least conceivable, even though false,

that this Y *is* an X. But surely it is inconceivable that a colour should be a Body? (pp. 58-59).

These difficulties suggest to Prichard a new and still odder theory, which he considers but rejects. I shall call it the 'Hyphenated Seeing' theory. When we imagine something, he maintains, there is no *object* of our imagination. 'When we speak of the Body imagined', *e.g.* a castle in the air, 'we are really only doing so to describe the special character of an act of imagining.' And something rather similar is true of desire too (p. 60). Is it possible that seeing is in this respect like imagining? When I say 'I see a tree' might I just be using the phrase 'a tree' to describe the special sort of seeing that it is? Then we could maintain that we do see Bodies after all, and this difficulty about putting the question once too often would not arise. 'Seeing-a-tree' or 'seeing-a-cat' would have to be taken as a single hyphenated phrase, as 'imagining-a-castle-in-the-air' has to be. I recall that when Prichard first propounded this view at a small philosophical gathering in Oxford, one of his senior colleagues, an idealist logician, remarked that 'Prichard was coming home at last'.

But Prichard did not come home; or if he did, he did not stay there long. By the time he wrote the present paper, he had made up his mind that this queer 'hyphenated' theory was certainly false. 'We all have to admit that a perception of any kind implies the reality of something distinguishable, though not necessarily separable, from the perception and constituting the object of it' (p. 61). And if so, the question 'what do we really see in what we call seeing a body' is not an illegitimate one after all.

Yet it 'still seems impossible that what we call seeing a body should really be seeing a colour, since this would involve the impossibility [*i.e.* the impossible process] of thinking or judging something which is not a Body to be a Body' (p. 61). Prichard now suggests that this puzzle arises from a false assumption: the assumption 'that when we are perceiving something, we are necessarily apprehending the character of what we perceive.' (*Ibid.* As the reader will have noticed, Prichard uses the word 'apprehending' to mean 'knowing in the active, as opposed to the dispositional, sense'.) But 'there is the possibility that we can perceive something without *apprehending* its character at all'. By 'its character', I think, Prichard here means what some people would call its ontological status. He is not merely pointing out that we can apprehend, say, a blue colour without noticing that it is blue. He goes on to suggest that in seeing something 'we do so say, *straight off* mistake it for something else' (p. 61, my italics)—'straight off in the sense that we do not attend to 'its character' at all. Now ordinarily, when we ask the question 'what did you really see?' we are assuming that the percipient did apprehend the character of what he saw and then judged or thought (rightly or wrongly) that it was, say, the back of Mr. Jones. If we withdraw that assumption, we cannot any longer

be accused of asking the question once too often. We are asking a different question from the ordinary one, because 'really see' no longer has the implication that what is seen is also apprehended. And we can now maintain with a good conscience that what we really see is spatially extended and spatially related colours.

In our ordinary consciousness, then, we are under a perpetual illusion. 'The thing which we take for something else, *viz.* a colour, must somehow manage to look more like the something else than like itself. It must, so to say, be a sham.' We may compare it to a forged ancient coin (p. 62). I think this is a somewhat peculiar sense of the word 'look'. In its ordinary use, 'looking so and so' is equivalent to 'visually appearing so and so'. Therefore (by Prichard's own doctrine) it can only be applied to Bodies, and to apply it to sensation-contents would be nonsensical. To use an example of his own, a field on a distant mountain side may look vertical. But the colour-expanse which the percipient senses (or perceives as Prichard would say) cannot look vertical, or look anything else. It *is* vertical; and indeed this is the analysis, in the Sensation Terminology, of the statement that the field—a Body or set of Bodies—*looks* so. I think then that Prichard in this passage is using the word 'look' in another sense, what may be called the Shamming sense, or the 'Pseudo' sense. If A 'looks' B in this sense, it is such that we immediately and without question take it to be B, though in fact it is not.

But even so, how do we come to take it to be B? How can a colour contrive to be a sham-Body, and such a very successful sham, that no one (except a few philosophers in their most philosophical moments) can avoid being taken in by it? One would think such a feat of deception impossible. A colour is a sensation-content, something wholly dependent on our seeing of it. Its existence is momentary. It cannot change, nor move, because it is not a substance. How can it be mistaken for something so utterly unlike itself, something of which none of these statements could be true?

To this Prichard answers that there is one important respect in which colours really *are* like Bodies. They are spatial. They have extension, shape, and location; and the space they are located in is *physical* space, the only space (according to Prichard) which there is or can be. In fact, he means by 'colours' not just qualities—red, blue, green—but what other people have called colour-expanses. Moreover, though he does not say so in this book, he certainly holds that they are three-dimensional—bulgy, recessive, located at different depths. I think he ought to have emphasised this; for unless it is insisted upon, his theory seems much more paradoxical than it really is.

Now since colours have those spatial characteristics, we can maintain 'that a certain colour would be *indistinguishable, so far as seeing goes*, from a certain coloured Body *if* there were such a Body and we saw it from a certain point of view' (p. 62, *ad. fin.*, my

italics). And this at least helps us to explain how the shamming is possible, and why it is so successful. Similarly, in touch we have feelings which are extended, shaped and located ; and this helps to explain why they too can be ' straight off mistaken ' for Bodies. (Prichard did not like using the word ' feeling ' in this sense, but says it is the best word he can find.)

Prichard makes one further point. If it is really true that we mistake colours and tactual feelings for Bodies, we ought to be able to show that the mistake, though habitual, is not inevitable. Having discovered, by philosophical reflection, that it *is* a mistake, we ought to be able to avoid making it. And Prichard maintains that we can, ' though only with some difficulty '. He gives the example of what we ordinarily call seeing clouds moving across the face of the moon. ' I do not think I am reporting my experience inaccurately when I say that if I make an effort I find myself thinking that I am seeing certain altering extended colours ' (p. 64). He is right in saying that effort is needed, and he might have added that many philosophers are reluctant to make it. He does not ask *why* effort is needed. The reason surely is that the mistaking—or anyhow the taking—of colour-expanses for Bodies is a process which has the very highest biological utility. We should therefore expect that there would be some repressive mechanism in the human animal which tends to prevent him from detecting a mistake—or anyhow a taking-for-granted—which is so important for his welfare.

Prichard sums up his theory by distinguishing three processes: (1) Perception proper. This is what other people would call sensing, or having sensations. (2) The mistaking of visual and tactual sensations for Bodies. This is what other people, from Reid onwards, have usually called perceiving. (3) A purely inferential process by which, on discovering the mistake, we infer what system of Bodies, including our own, is implied by process (1) as the cause of this process (pp. 64-65).

The reader would naturally like to know more about (3). But all Prichard tells him here is that it is ' one which we are constantly performing when confronted with what are called illusions ' although we have reflected very little about its nature (p. 65). For my part I very much doubt whether the ordinary man corrects his illusions by a *causal* inference. If it were so, he would have been incapable of correcting them at all before the sciences of Physics and Physiology were invented ; and no child could correct them now until he had attended a secondary school. Moreover, if we did correct our illusions in this way, our inference would already *assume* the existence of various Bodies, the percipient's own to begin with. If it is an open question whether there are any Bodies at all—as it must be, if our everyday perceptual consciousness is a form of mistaking—an inference of a much more drastic kind will be needed to settle it. Prichard says a little more about this inference in the lecture on Berkeley, as we shall see, but still he does not say enough. I

do not think he fully realised how urgent and crucial the question is for any theory such as his.

Indeed, the most important objection to his theory is precisely this, that if he is right our belief in the existence of Bodies *can* only be justified by a causal inference; and it is exceedingly difficult to see how such an inference could provide what is wanted, if its only premises are (1) the empirical fact that we have certain spatially-extended sensations, (2) the *a priori* principle that every event has a cause. Everything he has to tell us about the Phenomenology of Perception might be true if he had been content to say that we unreflectively *take* colour-expanses and tactual pressures for Bodies. The trouble arises because he insists, without any justification at all, that the taking is purely and simply *mis*taking. It is very odd, for example, that he concentrates his whole attention on individual acts of perceptual acceptance taken singly. He never asks whether a different situation does not arise when several such acts are considered together, as when we walk round a table and look at it from a number of different directions. Here we find ourselves taking a number of *different* colour-expanses for the *same* Body; and thereby, as we ordinarily believe, the existence of the Body is confirmed or verified, and its character is further specified. And illusions or hallucinations are detected—so we ordinarily believe—by finding that our initial 'taking' is *not* thus confirmable and specifiable. When Prichard discussed his theory with his colleagues in Oxford, they would often object that normal perceptions is not mistaking in the sense in which illusory perception is, and they would urge him to admit that some perceptual 'mistakings' are at any rate *less* mistaken than others. But I do not think he ever made his position clear on this point.

I now turn to the last paper in the book. 'The Sense-datum Fallacy', since this is a kind of appendix to the paper on Perception. Prichard held with Berkeley that the *esse* of colours, sounds, etc is *percipi*; or rather that it involves *percipi*, as he preferred to say. He offered no argument for this proposition. He maintained that it is self-evident (p. 207). And the Sense-datum Philosophy, he holds, is just the denial of this self-evident truth. The Sense-datum Philosophers assume, on the contrary, that perceiving (*i.e.* what they call sensing) is a form of *knowing*; from which it would follow that sounds, colours, etc. have an existence independent of being perceived. This inevitably leads them to ask questions which ought never to be asked at all, questions which presuppose the falsity of something self-evidently true. They are questions, Prichard says, about secondary qualities. For example, these philosophers ask what *sort* of a thing a colour or a sound is. Is it an event in the brain, or in the mind, and so transitory, or is it something permanent? Is it a substance, or a phase of a substance? If it is an event, how is it caused? Again, they ask whether colours, sounds, etc. are

private or public (p. 206), and they ought to answer 'public', though usually they do not. For if sensing is a form of knowing, then colours, sounds, smells, etc. must be independent of the sensing of them; and it must be *possible*, at least, that a colour seen by me should also be seen by you, even if it never in fact happens (p. 206). But if Berkeley is right, these are questions which do not arise at all. For 'since a sound, for instance, which we are hearing is inseparable from our hearing of it, it is something the hearing of which by another is impossible, and since there is no such thing as a sound which is independent of its being heard, there is nothing of which to ask "What sort of a thing is it?" and "How is it caused?", so that the very questions are mistaken' (p. 207).

It seems to me that Prichard is right about the *logic* of the Sense-datum Philosophy. The Sense-datum Philosophers do assume that 'perceiving' (*i.e.* sensing) is a form of knowing—what Lord Russell called knowledge by acquaintance. And many of the questions they ask only make sense if this assumption is true. (Of course, they may still consistently maintain that sense-data are brain-dependent events: or even that they are mind-dependent ones, provided that the mind-dependence is dependence on some mental process other than sensing. For instance, some of my sense-data might be different from what they are if my associations and habits of expectation were different from what they are.) Now the Sense-datum Philosophers may have been mistaken in assuming that sensing is a form of knowledge. They may have confused sensing with something else, namely phenomenological inspection of what is sensed; and moreover it might be argued that that is where the main utility of the sense-datum terminology lies, since without such a terminology (or something very like it) a phenomenological description of what is sensed is almost, if not quite, impracticable. But if their initial assumption is indeed mistaken, can we really show that its falsity follows from something self-evident? Is it *self-evident* that some sound which we hear is dependent on our hearing of it? For my part, I cannot see that it is.

Again, however queer the questions may be which the Sense-datum Philosophy leads us to ask, is Prichard right in saying that they are all questions about secondary qualities? If the phrase 'secondary quality' is used in Locke's sense ('a power to produce sensations in us') they certainly are not. And if the antithesis between 'primary' and 'secondary' is used in Berkeley's sense, to distinguish between qualities of sensation-contents themselves, it is equally clear that the Sense-datum Philosophers' questions are concerned with primary qualities at least as much as secondary ones. Otherwise, what is all this fuss they make about visual depth and visual shape, and about the properties of visual and tactual spaces?

So much for Prichard's chief objection to the Sense-datum Philosophy. But he also has another. People speak of sense-data

as if they were a *sort* of entities or events. But, he says, even if perceiving (*i.e.* sensing) *is* a form of knowing, there is no such sort. By 'a sort' we mean a set of somethings united by a common character. But sense-data, if *per impossibile* there were any, would not have any common character. 'If I am eating a number of things, they together form a certain numerical group *viz.* the totality of the things which I am eating. But their membership of this group does not constitute them . . . things of a certain sort. There is no such sort. . . . Indeed, to speak of *a* something which is being eaten by me or of *a* something which is eaten by someone is merely verbal, because to be" being eaten " is not a character of anything' (p. 211). For the same reason, it would be absurd to speak of *a* sense-datum *i.e. a* something sensed by someone.

I find this argument very odd. I do not see why these should not be *relational* characters, and why entities should not be grouped into classes by means of them. Will it be replied that such classes would not be *sorts*? If so, I wonder whether there are any sorts at all. For it is not easy to find any character which is purely and simply intrinsic to the thing characterised, and has nothing at all relational about it. Again, Prichard's rule for the use of the word 'a' is impossibly stringent. If he is right, it must be wrong to say that someone is *a* policeman or *an* undergraduate. Finally, I should have thought we could find one or two common characters in sense-data which are as nearly intrinsic as any characters ever are. All sense-data have what one might call sensuous vividness. They all have secondary qualities in Berkeley's—not Locke's—sense of the phrase. And these common characters are also peculiar to sense-data.

Prichard goes on to maintain that this objection of his does not apply to Berkeley's philosophy. When Berkeley says 'that the various secondary qualities which we perceive are sensations', he *is* attributing a common character to them (pp. 211 *fin.*-212). But what can this common character be? Prichard does not tell us. I think it either consists in the 'sensuous vividness' I mentioned just now, in which case the Sense-datum Philosophers are no worse, in this respect, than Berkeley; or else it consists in dependence upon a sentient mind, which is a character as relational as being known by acquaintance is, and then Berkeley is no better than they are.

The rest of the book consists of a set of lectures on the History of of the Theory of Knowledge, from Descartes to Hume (1932). They are interesting from many points of view, but I have not space to comment on them in great detail. Despite their title, they are not very historical. The object of studying these writers, in Prichard's view, was to help us to clear our own minds on certain fundamental problems, problems which are bound to confront us as soon as we reflect carefully on the process by which we come to

know. Thus he says of Locke, 'his is just the common-sense theory of knowledge which any able and thoughtful man would naturally produce, if turned loose to reflect upon knowledge for the first time' (p. 105). Our business, then, is just to ask whether the statements of these writers are true or false, whether their arguments are valid or invalid, and we need not bother ourselves overmuch with questions of historical background.

Naturally, anyone who discusses the History of Philosophy in this critical (rather than historical) spirit will give his own answers to the questions which Descartes or Locke or Berkeley raised. In examining their views, he will state the view which he himself believes to be true. And that is what Prichard does. His peculiar and rather rigid doctrines about the nature of knowledge itself are stated more fully in the lecture on Descartes than anywhere else. His theory of substance, elsewhere expressed only in rather cryptic *obiter dicta*, may be found in the lectures on Locke. In the same lectures he states his views on the nature of 'ideas' and especially on the distinction between simple and complex ideas. I shall now summarise what he says on these three important topics.

(1) Knowledge. We must begin, he says, by laying down two principles: that knowing differs in kind from believing, and that in knowing A we do, or at least can, know that we know (pp. 87-88). He thinks that Descartes, like many other philosophers, did not distinguish clearly enough between knowing and believing truly; and he points out that knowledge cannot be called true, any more than it can be called false. Anyone who uses the term 'knowledge' in so strict a sense must of course admit, as Prichard does, that most of what is commonly called knowledge is only correct belief at the best. It is therefore very important that he should give *instances* of knowledge in the strict sense. Prichard does so. I know that this noise which I am hearing is loud, though of course the hearing, as we have seen, is not in itself knowledge. I know that I believe it is made by a motor-car (p. 89). I know that I am wondering about such and such a question (p. 94). Again, in following a geometrical argument, I am knowing (p. 89). Prichard gives other and more controversial examples. He says he knows that Space and Time are infinite, and that space has three and only three dimensions (p. 98). Elsewhere, he says he knows that all motion is absolute (pp. 59-60).

It follows, from this conception of knowledge that the whole idea of a *criterion* of knowledge (whether Descartes' criterion or any other) is absurd. It is supposed that we are initially uncertain whether a given state of mind is or is not a state of knowing, and that we can decide that it is by applying a test. But to do so we must already *know* that the test is a sufficient one, and we must also *know* that this state of mind has the characteristics which the test requires (pp. 92-93).

Prichard proceeds to equate knowing with being certain. Of

course he can only do so by using the word ' certain ' too in a very strict and narrow sense. Thus, if it be said that we may be certain of something but mistaken, he replies that this confuses certainty with thinking without question (Cook Wilson's ' being under an impression that '). In thinking without question there is only absence of *un*certainty. Likewise, if it be said that there are mentally contradictory certainties, as when Jones is certain of p and Smith is certain of not-p, we may answer that at least one of them is only thinking without question (pp. 96-98).

Prichard finally turns to *a priori* knowledge. There might seem to be a special difficulty about this. No doubt there are necessities of thought. But do they apply to the real world ? There is something which necessitates *our* being certain that a three-sided plane figure has three angles. But how can we be sure that *in rerum naturâ* a three-sided plane figure is necessitated to have three angles ? To this Prichard replies that ' necessity of thought ' is a misleading phrase. What necessitates my thinking that A is B is just my knowledge that A, itself, is necessitated to *be* B. The trouble, of course, is that this implies or seems to imply that we have *a priori* knowledge of matters of fact, a difficulty which Prichard does not consider. I think he would have answered that it is only knowledge of necessary connexions, or disconnexions, of universals. But unfortunately his theory of universals, and of our knowledge of them, is nowhere fully stated—neither in this book nor in any other of his published writings. It is only clear that he took an uncompromisingly Realist view, and there are hints that it was of a Platonic rather than an Aristotelian kind. (*Cf.* pp. 112-113 of this book.)

(2) Substance. Prichard's theory of substance is stated in the lectures on Locke, pp. 131-40. He admits that Locke's difficulty about the knowability of substances is a genuine one. But he maintains that it is soluble, and he tries to show this by an ingenious *argumentum ad hominem*. The problem about ' supports for qualities ', he points out, also arises elsewhere. There are particulars which are not substances : for example, movements and sounds. They certainly have qualities and are not themselves qualities. Shall we say, then, that although we can know the qualities of a sound, we are incapable of knowing the sound itself, which ' supports ' them ; and that while we know what a velocity or a direction is, movements themselves, which ' support ' these characteristics, are unknowable ? Obviously not. ' Unless we knew what " a movement " . . . meant, we should not know what " a direction " or " a velocity " meant ' (p. 136). Thus, in these cases at least, ' the qualities cannot be apprehended apart from apprehending that of which they are the qualities '. The same argument applies to substances too. Locke tells us that a Body (a material substance) is the unknown support for such qualities as shape, hardness, elasticity. But unless we already knew what the phrase ' a Body ' meant, we should not know what was meant by ' shape ', ' hardness ',

etc. either. If you know what the qualities are, as you admit you do, you already know what the substance is, which has them.

Prichard, however, thinks there is still a difficulty about this relation of 'having', though none about the knowability of both its terms. And he confesses he is not sure of the solution. '*Possibly* the truth is that the relation . . . is one which cannot be stated in other terms, and though intelligible can only be understood in particular instances; and that the relation is intelligible in certain cases seems obvious from the case of a movement and a sound' (p. 138, Prichard's italics).

(3) Simple and Complex Ideas. Prichard's first point is that what is simple or complex is not the idea itself, but the reality of which we have an idea. Thus when Locke says that an army is a complex idea, it is an army (*i.e.*, I suppose, the characteristic of being an army) which is complex, and not the idea (p. 127). The second and more interesting point concerns Locke's theory of 'compounding'. According to Prichard, it is a complete mistake to suppose that complex ideas are *made* by the mind itself. It is not even true that we *make* complex images by putting simple images together. 'We may form the image of a church without a tower, or of a tower without a church, but if we form the image of a church with a tower we have not put together the previous images' (p. 129). And thinking, as distinct from imaging, neither is nor involves putting things together. 'What Locke calls combining A, B, and C is really *thinking of certain realities A, B, C as combined* or more strictly . . . as forming a unity' (*ibid.*, my italics). And they are thought of as combined in a particular way, not just in some unknown way, and moreover, 'in the particular way which their nature *necessitates*'. (I have italicised the word 'necessitates', because it is at this point that Prichard's own theory begins to be queer.) Thus what is called forming the complex idea of a triangle 'is the apprehension that three straight lines, such that they do not meet at a point and no two of them are parallel, must together form the boundary of a space, *i.e.* must have that unity which is indicated by the word triangle' (p. 129 *fin.* 130). It follows that the so-called combining of ideas is not an *arbitrary* process, as Locke implies that it is.

Prichard sees that there two objections to this. First, what about ideas of Natural Kinds, *e.g.* the idea of gold, of a metal, of an elephant? We think of certain qualities as combining to constitute the nature of an elephant. But plainly we do not know how they are connected. 'In fact, that is the chief thing we want to know about the realities to which we refer by the term "elephant"' (p. 130).

Just for this reason, Prichard replies, we do not even know that there *are* elephants. 'To the phrase "the idea of an elephant" there only corresponds the *belief* that certain qualities are connected or combined in reality.' And thus, 'there is no idea of an elephant in the sense in which there is the idea of a triangle' (pp. 130-131).

For in the case of the triangle we do have knowledge of a necessary connexion of characteristics, and in the case of an elephant we do not.

Secondly, do we not form ideas of imaginary entities, such as centaurs? Prichard replies that they are not ideas at all. For we mean by 'a centaur' a set of limbs, etc. which are physiologically, not just spatially, combined. And 'so far from having the thought of the combination of the parts, what we have is the belief [though not, of course, the knowledge] that there can be no such combination'. Therefore, Prichard concludes, 'we have no idea of a centaur'—not even in the sense in which we do have the idea of an elephant (p. 131).

I find this most paradoxical. It seems to me perfectly obvious that we do have an idea of a centaur. We mean something by the word 'centaur'. Prichard himself admits that we do; in fact he actually argues from its meaning. Again, although we do believe that there are elephants, it seems perfectly obvious that we could have the idea without the belief. When the duck-billed platypus was first described, people understood the description, *i.e.* had the idea of a duck-billed platypus, though they did not at first believe that so extraordinary a creature existed. And we have the idea now of a rocket-propelled missile capable of flying from Moscow to New York, though we have not sufficient evidence for either a belief or a disbelief in the existence of such an entity. Prichard has left no room for what is called the bare entertaining of a proposition. According to him, we must either know it to be true, or know it to be false, or believe it, or disbelieve it, and there is nothing further we can do. (Elsewhere, perhaps, he does leave room for bare entertaining, under the name of 'wondering', but not here.)

Moreover, it seems clear that there *is* something arbitrary about some complex ideas at least. Prichard, of course, is quite right in saying that this arbitrariness has limits (would Locke have denied it?). But he has drawn the limits in the wrong place. He has said 'necessary' when he should have said 'not impossible'. Instead of saying 'we think of the realities as combined in the particular way which we apprehend their nature necessitates' we should say 'in *one* of the ways which we apprehend their nature *permits of*'. And we must be careful to mean by 'what their nature permits of' what is logically possible; not merely what is causally possible, causal laws being what they in fact are. For example, we can conceive of physiological laws quite different from those which in fact prevail. Indeed, any physiological investigator does so when he entertains a physiological hypothesis which later turns out to be empirically false. Prichard is so devoted to Necessity that he has given Possibility less than its due. And in his Necessitarian zeal, he has restricted our liberty of thought too much. In effect, he has confounded invention with discovery (discovery of *a priori* truths at that); and he has laid an embargo on the free formation of hypotheses.

I shall not discuss the lectures on Berkeley and on Hume in detail. The ones on Berkeley cover much the same ground as the papers on perception, already discussed. There is, however, an interesting discussion on Touch (the previous papers were concerned almost entirely with sight) and some very acute criticism of Reid's views on the subject (pp. 164-171). Some further light is also thrown on the causal argument by which, according to Prichard, our belief in the existence of a world of Bodies is to be justified. It turns out to be an argument from *predictability*. From one perception, we are often able to predict others. If I see a boat moving down one part of a stream, I predict that I shall soon see it moving down a lower part of the stream. But the 'connection or coherence, belief in which underlies the prediction which we actually make, is essentially *indirect* and its connecting links are *physical* processes of certain kinds'—including, of course, physical processes in one's own senses organs (p. 172, my italics). Consequently, 'Berkeley, in appealing to the coherence of our perceptions as a condition of the power to predict which he urges we possess . . . actually implies the existence of the very physical world the existence of which he is denying' (p. 173).

But of course if this argument is to do what is needed it must have an *a priori* premise. And according to Prichard, we do know *a priori* that 'the various parts of our perceptive process, past and future [*i.e.* in such a case as that of the boat, above] must be due to a *single* cause' (pp 173 *fin.*-174, my italics). It is not at all clear to me how we know this, even if we do know *a priori* that each part of the perceptive process has some cause or other. Obviously the point needs more elaboration. But here, unfortunately, the lecture stops.

The lectures on Hume contain some very acute criticism, as we should expect. Thus Prichard points out that Hume, having abolished a permanent mind, is obliged to treat impressions themselves as persistent entities—as *things*, in fact (p. 191). He also argues that in the section of Scepticism with regard to Reason. Hume's sceptical conclusion depends on an illegitimate compounding of probabilities of different orders (pp. 192-196). But what is chiefly interesting in these lectures is not the detail, but Prichard's estimate of Hume's theory of knowledge as a whole. He is almost at a loss for words to express his disapproval. The *Treatise*, he says at the beginning, is 'one of the most tedious of books' (p. 174). At the end he speaks of 'the dreary process of considering Hume closely' (p. 199). He says of Hume's analysis of Necessary Connection, 'There seems no adequate epithet for it. It is an understatement to describe it as preposterous' (p. 189). He does not deny that there is a great deal of cleverness in the *Treatise*. 'But the cleverness is only that of extreme ingenuity or perversity, and the ingenuity is only exceeded by the perversity' (p. 174).

In the last paragraph of the lectures (p. 199) Prichard sums up as

follows: 'Hume speaks of his speculations as appearing ridiculous. So they do. And they *are* ridiculous.' But the cause of this 'does not lie, as Hume would have us suppose, in the irrationality of the mind and its incompetence to know. It lies in the superficiality of his own speculation, which leads him to describe the whole process by which we come to know . . . as if it were conducted by a mind in which all rationality had been left out.'

If Hume's theory of knowledge is so utterly worthless, why discuss it at all? Prichard had asked himself this question. 'It could be wished' he says, 'that the student of philosophy could be spared all contact with Hume' (p. 174). But unfortunately this wish cannot be gratified. 'There is a good deal of Hume in the natural man' [I think Prichard should have said 'in the natural English-speaking man'; I doubt whether there is much of Hume in the natural Frenchman or German.] He goes on 'the truth stands out better when placed in contrast with the opposing error, and in any case Hume's historical importance makes it impossible to ignore him. Moreover, there is a good deal of him prevalent in modern philosophy.' (p. 175; in a footnote Prichard mentions Russell's *Analysis of Mind*.) He adds later that 'there is a lot of Hume in Kant himself . . . much of the difficulty to which Kant's own position is exposed is due to his being himself tarred with the Hume brush more than he ever supposed' (p. 199). In short, the study of Hume is after all salutary, however distasteful. It is like 'having the measles and getting it over' (p. 199). It is fair to add that Prichard's attitude to Hume's moral philosophy was very different. He disagreed with it fundamentally, but in his later years at least he was prepared to treat it with respect, and almost with indulgence.

I will conclude by quoting some *oblita dicta* from the lectures on the History of the Theory of Knowledge:

'It is difficult to believe that it is fatal to be in a hurry, and that the truth cannot be set down on a few half-sheets of notepaper' (p. 70).

'You cannot make a man think any more than you can make a horse drink' (p. 99).

'Anyone in considering a specific problem would do well to begin by looking up what Locke has to say about it' (p. 105).

H. H. PRICE.

OXFORD UNIVERSITY.

IX.—NEW BOOKS

Aristotle : Prior and Posterior Analytics. A Revised Text with Introduction and Commentary. By W. D. Ross. Oxford : Clarendon Press, 1949. Pp. 704. 42s.

Sir David Ross's presentation of the *Analytics* is worthy company for his editions of the *Metaphysics* and the *Physics*. Every student of Aristotle will recognise what very high praise this is meant to convey. The scholarship, in the narrow sense, that has gone into the text must be appreciated elsewhere, and by specialists in it. It will be realised, however, that the choice of readings depends on an understanding of the writer's doctrines in a degree quite foreign to more literary works. That Ross has this mastery of Aristotelian theory and terminology at his finger-tips is vouched for by note after note of the Commentary. Particularly valuable are his accounts of the development and use of technical terms, where traditional misconceptions have sometimes to be corrected. As a commentator he gives the impression of being a little more austere than he was in the *Metaphysics* and the *Physics*. The reader of limited acquaintance with the Lyceum may more often be perplexed ; he is likely to miss here and there an excursus into theories which are not to be found in the *Analytics*. There are two notoriously hard chapters on causal inference in *An. Post.* (II, xi, xii). Ross concentrates on the difficult notion that causes can be regarded as parts of definitions. I do not think he makes it clear enough here or at any point that the book is concerned with science not as a method of discovery, but as the systems of theorems, where the freezing of the temporal element means also a transformation of the common sense idea of " cause ". (Scientific theories do not make predictions: they can be *used* in order to predict.) And another hard saying of Aristotle's, that we can infer neither a past nor a future event from a past event, would possibly strike some readers as less like sophistry if they were given some account in modern terms of the position in *Physics Z*.

This does not mean that Ross is not clear. To take another well-known labyrinth, in the proofs that there cannot be an infinite regress of premisses (*An. Post.* I, xxii) he has made the course of the argument intelligible as it has not, I think, been made before (although I think, incidentally, that he is down a wrong turning in 83 *a* 36-*b* 13). But he is clear within the limits he has set himself, and these are the framework of Aristotle's system and generally also of Aristotle's terminology. The result is in one sense an exposition of what Aristotle meant and in another sense only of what he said. It raises the question how far the activity of history of philosophy can be separated from the activity of philosophy.

There is a further question, which is often asked to-day. Is Aristotle's logic worth studying in any other way than as a period piece ? For it is arguable that it has been made quite out of date by the work of logicians in the last hundred years. (After all, it is reasonable to acclaim Boyle as the father of modern chemistry without expecting students to read him.) Now there have been many recent " defences " of Aristotelian logic. But since they seem to me to defend it only against relatively superficial objections, it may not be out of place to suggest (though only allusively and with some uncertainty) what is the fundamental trouble with it.

This is not that it is partial—does not extend to the sentential calculus, say, or to mathematical operations like multiplication or ratio. It is, in a word, that it is *incapable* of such extension. Wider logic is a different logic. And the reason is that the fourth century lacked the notion of a *variable*. It was therefore debarred from generalising a number of types of discourse. For, in modern jargon, it could not make one calculus reducible to another; it could not have quantification, still less the important conception of multiple quantification. (Compare the place of "oblique conclusions" in—or rather outside—Aristotelian logic (*An. Pr.* I, xxxvi) with their place as "plural descriptive functions" in *Principia Mathematica*!). No doubt it is possible to say "some" and "any" in Greek (although it is not so easy to say "any" without meaning "all"), and there is no Euclidean line between having and not having the notion of a "variable". But that Aristotle's "A", "B", and "C" are more in the nature of *abbreviations* than variables is apparent from the tragedy of Greek mathematics, which also used letters and knew conic sections—but knew no analytic geometry, no differential calculus, no algebra. And when Aristotle's brilliant and original analysis of the time continuum collapses on the point of being epoch making (in *Phys.* 219 *b* 22-33), the cause is the same shortcoming that makes him dismiss "predication *per accidens*" in the *Analytics* ("the white thing is a log" instead of "the log is white"). The latter error is less striking, but it ought in the twentieth century to be just as revealing.

Doubtless his metaphysics of substance and attribute would tend to inhibit Aristotle from understanding a true variable. But there is a broader streak of nominalism in the *Organon* than is commonly allowed; and his admiration for *generalisation* cannot be questioned. At least one can be sure of this: could he read some of his modern "defences" he would only think sadly, "*Non tali auxilio!*" Ross deals little with the question. But against the objection that Aristotle virtually confines himself to the logic of syllogisms he replies (p. 36) that, because it is possible, as it is not with relational arguments, to work out their forms exhaustively, it is "the fundamental part of the logic of inference. . . ." The least comment one can allow oneself is that this appears to be a *nonsequitur*.

It has, I suspect, to be concluded that the study of logic in Aristotle is unrewarding unless the study is deep and critical. This is specially clear when it is noticed how his survey of syllogisms is almost bogged under the muddy waters of modal propositions. His editor has gone a long way towards rescuing him here. He has explained step by step the consequences drawn from the definition of $\Diamond p$ as $\sim Np \,.\, \sim\sim Np$ (Np means "it is necessary that p"; the symbols are not used by Ross); and Aristotle's mastery of them emerges against certain criticism of Maier's. But since he seems to believe that the philosopher's virtues do not lie, like Coriolanus's, in the interpretation of our time, Ross gives no help to the reader who might pardonably suppose that he could gain a greater insight into both modern and into Aristotelian logic by using the techniques of the former to test the latter. Thus although the cumbersomeness of the definition is pointed out, its disastrousness is not. (It allows any proposition to be proved, since $\Diamond p \supset \sim \Diamond p$ will be a theorem.) On the other hand "Theophrastus' rule", that the conclusion follows the weaker premiss (in modality), is said (p. 47) to be vindicated against Aristotle's view. But since Theophrastus has just been complimented for prefering the definition (which became the traditional one) of $\Diamond p$ as

$\sim Np$, it is presumably meant that the rule holds for him as well; and indeed this has often been stated: but it is incorrect. For example, from

$$\Diamond \text{MAP} . \Diamond \text{SAM} \supset \Diamond \text{SAP}$$

and the accepted principle

$$p . q \supset r : \supset q . \sim r \supset \sim p$$

one gets

$$N \text{SOP} . \Diamond \text{SAM} \supset N \text{MOP}.$$

The subject of modal logic is of course extremely difficult. These are two illustrations of the dangers of any sort of intuitive approach to it even in Aristotle, and at the same time of its rewardingness for anyone who forgoes that approach—he may, for instance, be led to speculate whether a modal logic can be inserted as part of a two-valued system at all.

A. C. LLOYD.

Theory of Knowledge: an Introduction. By A. D. WOOZLEY. London: Hutchison's University Library, 1949. Pp. 196. 7s. 6d.

THIS book is a useful introduction to the range of philosophical difficulties called 'epistemological'. It deals clearly with 'direct' and 'representative' accounts of perception and memory, considers alternative doctrines of 'universals', and ends up with a good discussion of the 'coherence' and 'correspondence' accounts of truth and of the relations between believing and knowing. Its topics lie within the well-tilled territory covered by Russell's *Problems of Philosophy*: its methods and assumptions are also, in a very wide sense, Russellian. But it has given fresh moulds to thoughts which now form the classical basis of our modern essays in dissolution or reconstruction, and it has also made them much more resistant to contemporary controversial stresses.

A few indications will suffice to show the general trend of Mr. Woozley's exposition. He starts off by saying that the name 'theory of knowledge' is a misnomer, and that the situations studied by the epistemologist comprise *all* those in which we can be said to have some object 'present to our minds', whether such presence involves us in asserting or denying or remembering or imagining or questioning something. And he points out further, that, while the interest of the psychologist lies in finding causal explanations for the situations in question, the interest of the philosopher lies in clarifying their inner make-up, as well as in showing up their connections with the realities of nature and history. He then dwells on the considerations which have led philosophers to draw a distinction between what is strictly 'given' in experience and 'what we take that given to be', as well as on the considerations which have led them to draw a distinction between the 'propositions entertained in our beliefs, and the 'facts' to which these propositions correspond. He shows up the difficulties of a representative dualism in the special case of memory, and argues for the interesting, if curiously stated, thesis that, while images are necessary to memory situations, they are not to be regarded as entities: the word 'image' stands in fact for 'a certain mode of awareness, the way an object appears when it enters a memory (or imagination) situation'.

Woozley next passes on to the arguments which have led philosophers to treat general words as the names of peculiar entities, and to the difficulties engendered by this way of speaking. He himself comes down in

favour of a Stoutean form of expression, according to which the properties of a thing are private and peculiar to that thing, but can none the less be more or less like, or exactly like, the properties of other things, the things themselves acquiring thereby a different but derived sort of similarity. He demolishes objections to this resemblance doctrine that have been put forward by Russell and Price. He then runs through the familiar reasons that have led philosophers to speak in terms of objective 'propositions', and to distinguish these sharply from sentences, from judgements, and from facts. He objects to such talk for the curious reason that the subsistence of propositions isn't *empirically* verifiable or fruitful, and that, if there are such entities, there will have to be a very large number of them. Russell's account of believing as a multiple relation is suitably criticized. Woozley himself inclines to a modified proposition-doctrine according to which a proposition is that *part* of a belief by virtue of which the belief in question can be said to correspond to some fact. (Woozley doesn't go into the difficulty of saying that a belief may contain a self-contradictory and impossible 'part'.)

The relation badly expressed by the word 'correspondence' is then carefully examined. Good reasons are found for refusing to assimilate it to any form of mirroring or duplication, and Woozley comes down in favour of a sensible doctrine (which happens also to be that of Meinong) that a true proposition should be simply *identified* with a fact. Woozley then considers the nature of knowledge, and its opposition to belief. He sensibly rejects those rarefied accounts of knowledge according to which we ought never to say that we know anything, if there is even a *logical* possibility of our being mistaken. He maintains simply that we know something whenever we are right about it, and sure about it, and our evidence is conclusive. He doubts whether there is any general problem as to *when* evidence becomes conclusive, and he says that, if there is, we are not obliged to solve it in order to be justifiably sure that we *have* conclusive evidence in particular cases.

My real quarrel with Mr. Woozley's book is not, however, with his detailed decisions (which often seem to be admirable), but with his whole approach to the subject. For it ought surely to be clear, in this day and age, that the questions as to whether there are or are not such entities as images, or universals, or facts, or propositions, and how they may be related to each other, are not questions with plain meanings, which permit of straightforward answers, like the questions as to whether a child's spots are or are not symptomatic of measles (to which Mr. Woozley assimilates them in the first paragraph of his book). Such questions aren't framed in ordinary terms, nor in accordance with ordinary usage, and if they are anything, they are plainly requests for a new terminology, and for new forms of speaking, which we hope will deliver us from the 'cramps' and confusions bound up with certain common language-forms. This being the case, it is absurd to speak as if *one* answer to such questions could be the only right one, or as if we could ever do more than weigh up the advantages and disadvantages of different ways of saying things.

All these are points that Mr. Woozley frequently recognizes. For he says of the Platonic and the Aristotelian theories of universals that they are quite unintelligible if they profess to be *explaining* (and not simply restating) what we all mean, and perfectly understand, when we say that several things have some feature in common. But he doesn't seem to see that the same unintelligibility will apply to his own account of images

as ' the way in which objects appear when they enter the memory situation', or of propositions as ' parts ' of beliefs, which may in certain cases be ' numerically identical ' with facts. Regarded as ways of speaking which help us to guard against certain insidious and facile dualisms, such ' theories' are helpful and illuminating : regarded as explanations of what we all know and accept, they are wholly senseless and unprofitable. All this would hardly have been worth saying had Mr. Woozley's book been simply written in the naively ontological idiom of previous generations. It is because it goes so far in what seems to me the right direction, that I can't help regretting that it doesn't go further.

J. N. FINDLAY.

Language and Philosophy. By MAX BLACK. Cornell University Press. London : Geoffrey Cumberlege. O.U.P., 1949. Pp. xvi and 264 £1 8s.

THIS book contains ten essays first published between 1937 and 1949. Two of them (" The semantic definition of truth " and " Wittgenstein's Tractatus ") appeared in English periodicals, one (" Russell's philosophy of language ") was contributed to " The philosophy of Bertrand Russell " and the others, which are likely to be new to English readers, were contributed to American periodicals or else receive their first full publication now. There are also some additional notes on comments that the separate essays provoked from other philosophers. Though the sub-title of the book is " Studies in method ", Professor Black admits in his preface that there is still too much disagreement about methodology to hope for any general acceptance of a systematic philosophy of language. He has tried mainly to use any ideas about language which will help to clarify philosophical problems, but there are signs that he would welcome the emergence of a definitive critical method. Two of the essays (" Linguistic method in philosophy " and " The justification of induction ") are largely exercises in locating what Black calls the use in a limiting sense of terms such as ' colour-blind ' or ' justification '. In this use the term becomes, he says, either vacuous because all rules for its application have been abandoned, or self-contradictory because conflicting rules have been introduced. Although Black's pursuit of such terms in sceptical arguments used as examples is illuminated by important detail, he seems to think that once he has established that the argument rests on a limiting sense the philosophical job is finished. I do not feel certain that all such arguments are equally worthless, and it seems possible that study should also be made of the particular ways in which the limiting senses are reached, not only with a view to practice in the detection of limiting senses, but also in order to find out whether philosophically one limiting sense need not be distinguished from another.

Another group of essays (" The semiotic of Charles Morris ", " Ogden and Richards' theory of interpretation " and " Questions about emotive meaning ") consists of critical studies of various more or less causal theories of meaning. These are particularly interesting in that they reveal Black's ambiguous approach to causal statements about meaning. Ogden and Richards are said to have promulgated " an empirical theory inadequately supported ", whereas the use of terms like " condition " and " significatum " in Morris's semiotic is supposed to involve reference to a metalanguage,

in which case "semiotic . . . could hardly be a descriptive science". Of course it could be argued that the two theories do not have the same subject matter at all, but I do not think Black would go as far as this. He seems not to be quite certain which questions are straightforwardly empirical. A further investigation of the complexity of the metalanguage necessary for describing a semiotic like that of Morris would, as Black suggests, deserve more attention than the proposal has received. Unfortunately Black's observations on this subject are more or less restricted to admitting that he regards the word "represent" or some synonym as indispensable to any satisfactory analysis of symbolism, although he recognises that a theoretical account of the use of such a term is extremely difficult.

The essay on vagueness proceeds on the assumption that variations in the application of a word to a specific situation by a group of people exhibit some statistical regularity. A set of complementary definitions of "users of a language", "a situation in which a user of a language is trying to apply a symbol L to an object x" and "the consistency of application of L to x" is proposed. This leads on to a version of the syllogistic law in which terms of a measured degree of vagueness are used. Ingenious as this treatment is, the claim that "the wholesale destruction of the formal sciences" has been avoided by this analysis seems exaggerated. These techniques, which may have a future application in applied logic, are interesting enough on their own account without the claim that they remove a supposed need for regarding vagueness as a defect of language.

The remaining essays include a revision in the formulation of Russell's theory of types and a criticism of reducibility to acquaintance, a defence of the Tractatus from some misinterpretations, a brief exposition of Tarski's semantic definition of truth and a denial that any definition could be an illuminating and direct answer to the philosophical problem of truth. Finally there is an account of Korzybski's general semantics. If the account does justice to Korzybski the criticism that follows is very temperate. The book as a whole, although Black tends not to elaborate its most interesting points, contains a great deal of stimulating discussion.

O. P. Wood.

The Philosophy of Thorstein Veblen. By Stanley Matthew Daugert. King's Crown Press (London: Geoffrey Cumberlege), 1950. Pp. x + 134. 18s.

As a young man Veblen deliberately transferred his attention from philosophy to social science. The only work of his which survives from before this transfer is an article on Kant's *Critique of Judgement*, which was of more interest to the American reader of 1884 (when the third *Critique* had not yet been published in an English translation) than it is to the Anglo-Saxon philosophical public of to-day. But he had read and thought widely in philosophy before he came to devote his attention to sociology and economics, and in pursuing these studies he was always methodologically self-conscious.

Mr. Daugert has produced a book on the "philosophy" of Veblen by grouping together under that rubric a number of somewhat disparate theories. In his first chapter he deals with the lesson which he thinks Veblen learnt from Kant, though I suspect that he may be exaggerating

the importance of Veblen's early interest in Kant. In his second chapter (which is philosophically the most interesting one) he discusses Veblen's theory of sociological method. In the third chapter Mr. Daugert examines the psychological generalisations which underlie Veblen's sociological and economic theories. And in the fourth chapter he investigates what Karl Mannheim would have called Veblen's "sociology of knowledge". On these particular subjects those who have no time for the original writings will find an accurate summary in Mr. Daugert's book, with copious quotations from the relevant texts and without the complications of any attempt at a critical appraisal of Veblen's work in these fields. But those who seek a general summary of Veblen's always stimulating theories are better advised to read J. A. Hobson's *Veblen*. Those who seek a comprehensive study of the background and development of Veblen's thought must still go to J. Dorfman's *Thorstein Veblen and His America*. And those who want to enjoy Veblen must read the *ipsissima verba*, repetitious though they often are.

JONATHAN COHEN.

The Philosophy of Plato. By G. C. FIELD. Home University Library; Cumberlege: Oxford University Press. Pp. 220. 5s.

As we should expect from the author of *Plato and his Contemporaries*, Professor Field has given us an admirably lucid, balanced and stimulating introduction to Plato's philosophy. The book, after an introductory chapter which sets the historical scene, falls into three main sections. In the first, an account is given of the Theory of Forms; Professor Field differs from some scholars in considering the *Parmenides* to be of much less importance for the development of Plato's thought than has been maintained. Parallels are quoted from the more philosophical writings of some modern scientists which make the Theory of Forms seem more up-to-date than a first reading of Plato might suggest. The second main section is devoted to Plato's moral and political theory, and shows how, by care in translation and interpretation, Plato's views can be rendered considerably less outrageous to modern sentiments than has been held. The third and longest section is devoted to later developments in Plato's thought, the theory of the soul, the final metaphysic, and his religious views. There follows a chapter on miscellaneous questions, and another called "Plato Today", which contains, as well as some admirable advice to the modern reader of the Dialogues, a spirited defence of Plato against some modern attacks.

It is to be regretted that Platonists generally have not made much use, for the purpose of a sympathetic study of Plato, of the methods of logical analysis which are now available. There are certain passages which suggest that Professor Field is no exception. For example, Plato's Theory of Forms represents an important logical discovery in ontological dress, and it would be foolish to quarrel with the dress if it helped Plato to make the discovery, just as no logician should grudge the earlier atomic physicists their "models" of the atom. But it is a very poor defence of Plato against the common criticisms to insist, as Professor Field seems to, on the reader swallowing the theory, clothes and all; to do this is to discount the work of all the logicians from Aristotle to Carnap. If Plato in the fourth century B.C. used the material mode of speech, there is no reason

why we should not now avoid the confusions which result therefrom. Again, when Professor Field says (p. 30) " I do not think, for instance, that I have ever actually seen what I suppose to be the real shape of the surface of my table " he raises a problem which can be elucidated only by a study of the word " real " as it is ordinarily used—a study towards which Plato's contributions, though paradoxical, are not negligible. Not enough is done in this book to stress the importance of such considerations, and as a result Plato is left inadequately defended against the most serious charge of his modern critics, that he surreptitiously twists ordinary words into new meanings. On p. 28, too, where Professor Field says " All our thinking and investigation from the first is an attempt to find out what is real or what really is the case ", he outdoes Plato in ignoring that very large part of our thinking which is directed towards deciding what to do.

Useful as it will be to many, especially to those taking up the study of Plato for the first time, the book, like others of the same series, suffers from a serious defect, the almost complete absence of verbal quotations or even page-references. In this respect it suffers by comparison with Taylor's small life of Socrates. This omission is bound to have many adverse effects. In the first place, the beginner, faced with this book and the very different account of Plato given by Professor Popper, is likely to believe the writer who quotes chapter and verse. Even for a reader who is acquainted with the Dialogues, it is very hard without the references to decide just where interpretation ends and whitewashing begins; for example, on p. 176, where it is said of the Republic, " . . . there are some reasons for thinking it possible that the early stages of the basic education would be common to all classes ", a few references would be a kindness to the reader who wants to weigh further this much-disputed suggestion. To omit such aids to verification is to set oneself up as a barrier between one's reader and Plato. It is to be sincerely hoped that this defect may be remedied in a later edition, and that thus the fruits of Professor Field's wide scholarship may be made more accessible to those who wish to go on from this book to Plato himself.

R. M. HARE.

Platon. By ERNST HOFFMANN. Artemis-Verlag, Zürich, 1950. Pp. 222. (10 francs—about 17/6).

THIS attractively produced book contains 14 lectures originally delivered in 1946-47 to students of all Faculties at Heidelberg University, together with some 30 pages of notes. It is no conventional introduction to Plato: it is not easy, it is highly unorthodox (without mentioning the fact), it ranges so widely that a fair acquaintance with the dialogues is required for a really profitable reading. Serious students of Plato will regret the lack of detailed analysis and of arguments in support of the views put forward, and they will find the treatment somewhat unsystematic. But they will recognise here an enthusiastic and stimulating study which comes to grips with serious problems. Professor Hoffmann makes a large number of interesting suggestions and neat (sometimes over-neat) formulations. His central doctrines are highly dubious, but neither silly not dull. His style is winningly clear and pleasant.

It is commonly remarked that in his later dialogues Plato becomes less

preoccupied with the theory of Forms and more and more interested in the nature and status of Soul—the dynamic factor so noticeably missing from the original 'two-world' theory. Life and movement are real (Sophist 248e). The soul can be defined as that which moves itself (Phaedrus 245, Laws X, cp. Timaeus, 46 e 1). Its proper movements—wishing, deliberating, believing, rejoicing and the like—are prior to all other sorts of change and 'rule over them'.

Hoffmann maintains that in *all* the dialogues that contain Plato's own philosophy (namely from the Gorgias on) the need for a dynamic factor is recognised: "Plato's philosophy was in all its phases Dialectic, but it was at the same time something else as well—an Eros-doctrine. Just as there is Chorismos *and* Methexis, so there is Logos *and* Mythos, theory of Forms *and* Eros-doctrine, . . . and this is because beside the ontological problem stands the dynamic. Alongside the philosophy of Being is the philosophy of Soul. And I regard as the essence of Plato this intertwining of the problems of Being and Soul." (p. 62.)

The first and most important step in reaching this view is to identify the Form of Good with God ("Plato's is a fundamentally theistic philosophy"), and to add also features from the One of the Parmenides and from the Demiourgos of the Timaeus. The other Forms are causae exemplares, the Form of Good (= God) is the causa existentialis. It is responsible for the existence and value of the other Forms, which represent it, each in its own partial way, and which are striven after as models just because they are *good;* it is also that which gives to the empirical world its life, its power to participate in the Forms. As a Form the Good is, like the other Forms, transcendental; as the sole ultimate dynamic force God is "immanent in Becoming".

This much, according to Hoffmann, is already to be found in dialogues of the middle period. Of later dialogues he treats particularly the Sophist and the Phaedrus. The realisation in the Sophist that the Forms share in each other in certain definite ways is the realisation that they—the world of Being—constitute a "truly living and organic" whole. Their life consists in their non-spatial, non-temporal movement, that is to say, in their eternal relationships one with another. The Phaedrus brings the definition of the soul as that which moves itself. "In a way analogous to that in which he had reached the doctrine of the Forms as Relations Plato came to the conception of knowledge as the non-spatial movement of the rational soul. In this self-movement consists the essence of the soul, which distinguishes it from everything which is merely corporeal and entirely subject to ἀνάγκη. In the movement between 'Yes' and 'No' in judgment, in the movement from Subject to Predicate and from sense-perception to pure thought . . . , in this spiritual "Coming into Being" lies for Plato that which alone can be intelligibly described as spontaneity, auto-kinesis, first principle' (p. 106).

We can now say that Soul is the dynamic factor—it "represents the function of God in the created Cosmos". And we can now see that the power empirical things have to share in Forms—their life—is just the Soul's power to move itself in its effort to participate in the Forms. "There can be no methexis without Soul". Life and knowledge both share in Forms in the same way. The methexis of the middle dialogues was a striving due somehow to God: that of the later dialogues is explained by the Soul's spontaneous efforts to enter into its inheritance in the world of Being.

These are perhaps the most characteristic central theses of this book, though not necessarily its most valuable part. Hoffmann has stimulating comments to make on the Platonic dialogue as a literary form, on the relations between Plato, Aristotle and Plotinus, on the Cave and the Line, on some of Plato's predecessors and contemporaries and on several dialogues (though there is curiously little about the Timaeus and hardly a reference to the Philebus or Laws).

Whether Hoffmann is right in his initial identification of God and the Good (not to mention the One and the Demiourgos) cannot be discussed here. But it is worth pointing out how in two other important doctrines he certainly is guilty of neglecting important distinctions and of treating analogies as if they were equivalences.

1. *Methexis.*

Hoffman's attempt to make Plato's philosophy consistently dynamic and to expound the later Soul-doctrines as mere explication and development of earlier God-doctrines leads him to a serious contamination of two senses of 'methexis' which are not so confused by Plato. To say that *x* participates in the Form Circle may be to say that *x* is an instance of Circularity. This is the main meaning of 'participation' in Plato and comes into the attempt to solve logical puzzles about classes and general words. The actual term methexis is by no means essential. We could equally well say that Forms are 'present to' their instances (Phaedo 100d). On the other hand, to say that *x* participates in the Form Circle may be to say that *x* thinks about, contemplates or knows the Form. Clearly only rational beings are capable of participation in this sense: and they are in principle capable of sharing in the *whole* world of Forms. The Socratic view that really and truly knowing the Form Courage entails being courageous does imply that in some cases both types of participation must be present together. But this is not normally so. The mathematician (or dialectician) contemplating the Form Circle does not thereby share in it in the sense in which particular perceptible or mathematical circles do.

Hoffmann blurs this distinction by an indiscriminate use of the notion of striving in connection with all phases of Plato's thought. This only makes confusion worse confounded. For, in the first place, it is surely not the case that both sorts of participation involve striving, let alone striving in the same sense of the word. When Plato speaks of *e.g.* a pair of sticks as trying to be equal or as striving after the Form Equality he speaks metaphorically and to stress that particulars (except τὰ μαθηματικά) inevitably fall short of perfection. Nor is it clear to what extent the contemplation or knowledge of Forms necessarily involves striving. Hoffmann's idea may be that increasingly adequate knowledge of a Form is accompanied by increasingly complete 'embodiment' of it ("Life and knowledge both share in Forms in the same way"). But this can be true only of a very limited group of Forms, notably ethical ones. Secondly, even if all rational apprehension of Forms were in some way—or were always connected with—a striving to 'embody' or 'realise' them, it would by no means follow that this was the only kind of dynamic striving. Plato gives us a doctrine of Soul as the source of life and movement, Hoffmann a doctrine of Nous. Plato ascribes to Soul all sorts of movements beside thinking (Laws 896 e): it is responsible for bad as well as good (Laws 896, 904 b): it may act either νοῦν προσλαβοῦσα or ἀνόᾳ συγγενομένη (Laws 897 b. Cp. 898 b, Epinomis 988 e). Hoffmann is wrong to identify the

autokinetic power of the Soul with thinking (p. 106 quoted above). Having done so, since he at the same time regards it as the dynamic factor, the source of all striving, he should conclude that *all* methexis of particulars in Forms is or involves an intellectual activity. This conclusion he does not draw and obviously does not wish to draw (p. 101): but to avoid it he ought to make drastic alterations in his general treatment of methexis and autokinesis.

2. *The Sophist.*

It seems to me that Hoffmann errs over methexis from an undue desire to produce a single Platonic philosophy. He does not allow sufficient weight to the fact that in different dialogues and at different times Plato was pre-occupied with different questions, and in particular he under-estimates the importance of purely logical questions in the middle period. Similarly in the Sophist he strangely under-emphasises the crucial logical problems involved and the tremendous discovery that there are certain specially pervasive Forms linking the rest. Instead, the fact that the 'articulation of parts' is a mark of the organic (Phaedrus 264 c) is taken to mean that the world of Forms has now been discovered to be living: its life is non-spatial, non-temporal autokinesis, its pattern of internal relationships. But surely if this is what 'life' means we need another word to describe living dogs and ducks: and if this is what 'autokinesis' means we need another word for the spontaneous activities of the Soul (which are not non-temporal or even exclusively intellectual). Hoffmann himself draws these distinctions *en passant*. He writes: "Life is Becoming striving after Being" (p. 109). In this case Being itself cannot be living. Again: "Absolute Being has its own movement, for it is living, but not a movement of striving, for it is itself the End" (p. 118). Yet he nowhere admits that to ascribe life to the world of Forms is at best to over-stress an analogy and at worst to be trapped by an ambiguity. It is true that Plato himself sometimes misuses analogies, but not in this case. Sophist 248 e is an admission that movement (in the common or garden sense) really occurs, not that the Forms themselves have motion or life. And the *νοητὸν ζῷον* of the Timaeus is not "the eternal Cosmos, the true fellowship of the Forms" (p. 186, Timaeus 30-31).

In many other places minor inconsistencies and obscurities are noticeable, some of which are due to a certain Platonic indifference to terminological exactitude. Enough has been said to show that this book needs to be read with care. It fully deserves such a reading.

J. L. ACKRILL.

The Unwritten Philosophy and Other Essays. By F. M. CORNFORD. Edited with an introductory memoir by W. K. C. GUTHRIE. Cambridge: The University Press, 1950. Pp. xix + 139. 12s. 6d.

OF these eight essays by the late Professor F. M. CORNFORD, some are attempts to interpret Greek theories for the general reader, while others discuss the unconscious assumptions and traditions of the Greeks which must be realised before proper understanding and interpretation are possible. They are attractively written and contain many stimulating suggestions even for the specialist, but brevity of treatment often prevents their being fully convincing. Five of them have not been published before and none has appeared in a philosophical periodical.

In the first essay, "The Unconscious Element in Literature and Philosophy", Cornford attempts to clear up misunderstandings about his earlier work and discusses primarily the unconscious influence of the traditional myths on Greek writers, for which influence he offers a (dubious) psychological explanation. This short essay is hardly adequate in itself, but Guthrie, in his introduction, sketches Cornford's general views on the "unconscious element" and gives references to the books in which they are fully developed. Better and with a similar theme is the third essay, "The Unwritten Philosophy", in which Cornford considers whether there are any special principles to guide us in reconstructing and interpreting ancient philosophies, and gives an interesting but all too brief discussion of some basic assumptions of Greek philosophers. The second essay, "The Harmony of the Spheres", is an at times fanciful account of Pythagoras's theories which tries to do justice to their emotional and aesthetic aspects. The fourth and fifth are sympathetic introductions to some of the main doctrines of the *Republic* and the *Symposium;* while the sixth gives a good account of the differences between the Greek and modern approaches to physical science, although there is the danger in thus limiting the essay to physics that the reader may be led into generalisations about Greek science which neglect the amount of observation done by *e.g.* Hippocrates or Aristotle. The seventh, "A Ritual Basis for Hesiod's Theogony", is more technical than the rest and contains little of philosophical interest. The last essay, "The Marxist View of Ancient Philosophy", is chiefly (i) an acute criticism of Farrington's eulogy of the Ionian philosophers, and (ii) a vigorous defence of Plato against the charges of Farrington, Thomson, and others, that he was a reactionary oligarch advocating a totalitarian class-divided society stabilised by false propaganda etc. This essay is sometimes too indulgent to Plato and was written before Popper's more circumstantial attack.

R. J. HIRST.

Christianity and History. By HERBERT BUTTERFIELD. B. & U. 7s. 6d.

PROFESSOR BUTTERFIELD, in the first of these lectures, distinguishes between, on the one hand, what he calls 'technical history' which is concerned with 'the mechanism of historical processes, the tangible factors involved in an episode . . . '; and, on the other hand, 'our total attitude towards the whole of human history'—this we may find 'in a Christian interpretation . . . or in the Marxian system, or even perhaps in H. G. Wells' "History of the World" '. The two are different and, one is at first led to suppose, unrelated.

Whatever difficulties there may be in the *statement* of this distinction, (the suggestion that an interpretation is a kind of mould clamped down upon 'facts' ready waiting to be 'interpreted' seems to me misleading), it is, broadly speaking, clearly important. And Professor Butterfield might have confined himself to writing a book simply designed to show what is involved in regarding history from the Christian standpoint. He does not, however, so confine himself—not only discussing history within the context of his own interpretation, but also moving outside it, comparing it with other interpretations, and so forth. And in doing this he raises problems of another character.

It is not in any case always certain how seriously Professor Butterfield takes the distinction emphasized in his first lecture. We are told, for

example, that the disaster which recently overtook 'Prussian militarism' is 'a specimen case' of 'the operation of the moral factor in history' and that it represents 'a valid example of moral judgement *within the terms of history itself*' (italics mine). This suggests that historical events themselves prescribe norms and mete out blame and punishment, which is odd. Equally odd is the use made of the concept of 'a providential order of things'; it is seldom clear whether it is being used (1) to refer merely to the way in which things, as a matter of fact, happen (2) to 'picture the course of things as if its final shape were under the direction of a superintending intellect', (as a Kantian 'Idea of Reason'), (3) to allude to the actual part played by God in the working out of the historical process.

Further obscurities occur in the discussion of the time-honoured question: 'What is history?' Here are some of Professor Butterfield's similes, culled from successive paragraphs. History is described as 'a live thing' which may 'break away from the railway lines which prophets and pedants have set for it', as a musical composition whose course is 'unpredictable', as a 'complex fabric' that is 'light as gossamer', and as being of an 'unspeakable liquidity'. Pictures do not always illustrate; and when at one stage history is spoken of as 'herself standing up and taking a hand in the game' this reviewer for one finds himself at a loss.

Perhaps such hypostatization has something to do with the vagueness—often-baffling—that comes to surround the account of what constitutes an historical interpretation, Professor Butterfield says that the Marxian formula 'thesis-antithesis-synthesis' represents a 'myth' or 'pattern of essential truth', which is 'better' [in what respects?] than 'the generally accepted view of linear development'. Toynbee's 'challenge and response' is likewise a myth 'so wonderful in its elasticity that . . . we can apply it almost anywhere in history'. And so with 'the conception of a Renaissance'. All these are 'formulas for . . . something . . . essential, something . . . at the roots of history.' Unfortunately, little is suggested regarding the logical status to be assigned to such 'formulae', and what *is* said about them—that they are not open to possible falsification, can be applied anywhere—is difficult to reconcile with the claim that they possess 'essential truth'. It would also be interesting to know the nature of the relation between such 'myths' and the 'interpretations' mentioned elsewhere. If it is one of identity, or of part to whole, does this square with the distinction initially drawn between 'technical history' and its interpretation? (For example, it is stated that Marxian historians have 'contributed more to . . . historical scholarships . . . than the non-Marxists like to confess'.) And is it in any case true that the Marxian and Christian interpretations are strictly comparable? An important feature of the former seems to be the recommendation that in any historical explanation (of the *causal* type) priority of place should be assigned to certain empirical factors falling within a definite range. And I can find no obvious parallel to this in Professor Butterfield's statement of the Christian interpretation.

Those are some of the problems the lectures raise and leave unresolved. There are others. It would be a mistake, however, to give the impression that the discussions which prompt them occupy the major part of the book. Much of it is concerned simply to expound and illustrate the outlook and perspective of a practising Christian historian: as such it is often stimulating and illuminating.

PATRICK GARDINER.

Reflections of a Physicist. By P. W. BRIDGMAN. New York: Philosophical Library, 1950. Pp. xiv + 392, price $5.00.

THIS book is a collection of twenty-two comparatively non-technical papers on various subjects, which Professor Bridgman has written in the years since the publication of his important and illuminating book, *The Logic of Modern Physics*. About a quarter of them are further analyses of problems arising in (and out of) the physical sciences. These are helpful and interesting, notably the two entitled 'Statistical Mechanics and the Second Law of Thermodynamics' and 'On the Nature and the Limitations of Cosmical Inquiries': the latter is timely in view of recent uncritical attempts to popularise physical cosmology.

Of the other papers, one (misleadingly called 'Freedom and the Individual' discusses a genuine and difficult philosophical problem, and well illustrates the dangers of transferring the 'operational method' to other branches of philosophy without modification. Let me quote the key steps in the argument:

> "What I do to establish that you have senations is different from what I do to establish that I have sensations, and since what I do is different, the meaning is different. (64) Thus, 'we feel happy' is a contraction for 'You feel happy and I feel happy', with a different meaning for 'feel' and 'happy' in the two parts of the sentence. (68) Because our language makes it difficult to see the dichotomy of meaning we find it almost impossible to realize . . . the essential isolation of each of us from his fellows. (69) There is no possibility of continuing to feel that one is in a *sympathetic* world . . . after one has seen that it does not make sense to say of even his fellow human being 'He has feelings like mine'. (78)"

What is curious here is the way in which the logical distinctions Professor Bridgman has seen get misinterpreted, one by one, as factual distinctions, so that his initial and exaggerated logical identification of 'meaning' with 'operations' comes in the course of fifteen pages to be taken as providing sufficient justification for a pessimistic attitude to the world.

The remaining papers present a sad spectacle, becoming progressively more confused the wider the author casts his net. One certainly does not get, from these extensions of his method, what is forecast in the preface: "a conception of the relation of the individual to society and of the society to the individual which affords that complete refutation of the totalitarian ideology for which we are so earnestly groping in this era" (viii): one does not even get a dispassionate analysis of problems in the social sciences. Instead, we are given a number of occasional pieces, largely concerned with appeals for higher pay for physicists and other local political issues, which throw little light on anything except the author—and, incidentally, on American life. On this side of the Atlantic, Professor Bridgman's political views, especially on the grading of income-tax, read a little oddly. Can anyone here see, in the suggestion that £9,000 a year after taxation is enough, even for the most intelligent of men, "as keen an affront to my self respect as ever a militant suffragette smarted under, denied the right to vote simply because of her sex" (337)? Do people here still feel that the fight against extremes of economic inequality is fairly described as 'envy masquerading as justice' (340)? One is sorry, but hardly surprised after reading such passages, to find the author time and again seeing in the contemporary political scene 'sinister' influences, 'sinister' developments and 'sinister' consequences.

STEPHEN TOULMIN.

The Greek Philosophers from Thales to Aristotle. By W. K. C. Guthrie. London: Methuen, 1950. Pp. vi + 168. 5s.

Mr. Guthrie's brief account of Greek philosophy down to Aristotle is 'based on a short course of lectures designed for an audience of undergraduates who were reading any subject other than Classics'. He has produced a successful and pleasant piece of work, clear and accurate and perceptive; and the need for brevity has not betrayed him into indigestibility. The most successful chapters are those on Aristotle.

The professional philosopher, though this book is not intended for him, can get a good deal out of it both as critic and as learner. As critic, he will find interest in examining Mr. Guthrie's use of the dualism of matter and mind in his account of the Presocratics, and his use of terms like 'moral anarchy' and 'absolute moral standards' in his exposition of Plato. As learner, any philosopher who reads these pages will find in them some illuminating remarks and some good quotations which he has forgotten; he may keenly regret the references, which Mr. Guthrie has suppressed.

Richard Robinson.

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DR. A. C. EWING, University of Cambridge, will represent the Mind Association at the above Congress, and is travelling to India as the guest of the Executive Committee of the Indian Philosophical Congress.